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Delegating Disclosure and Production Choices

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ABSTRACT: We study how joint delegation of production and disclosure choices alters the incentives that firm owners offer to their managers. Our first set of results shows how the incentive weights that owners place on revenues are affected by firm characteristics and by whether their manager chooses *ex ante* voluntary disclosure. This arises because the owners choose how sensitive the manager's compensation is to her production choice and, because this sensitivity is naturally greater if the manager opts to disclose, owners substitute disclosure for direct contractual incentives. Owners also substitute a rival firm's disclosures for direct incentives. Finally, we show that joint delegation alters the information environment for competing firms by creating incentives to provide more information about the less aggressive competitor and less information about the more aggressive competitor. All of these effects are exacerbated in industries with less product differentiation.

Keywords: *delegation; voluntary disclosure; Cournot competition; product differentiation.*

I. INTRODUCTION

A key characteristic of the modern firm, especially multinationals, is the separation of ownership and control and the associated requirement that operating and disclosure decisions be delegated to managers. The benefits and costs of such delegation have received significant attention in the theoretical literature in accounting and economics. A key finding in this literature is that delegation of production choices to a manager may result in direct financial benefits to the delegating owners of the firm.[1] Intuitively, these benefits may arise because

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[1] Most of the rest of the literature on delegation focuses on moral hazard and/or adverse selection issues associated with delegation, i.e., agency costs. The literature on agency costs is too large to cite exhaustively, but classic articles in this literature include Holmstrom (1979), Grossman and Hart (1983), and Holmstrom and Milgrom (1987). See, also, textbook treatments such as Laffont and Martimort (2001), Bolton and Dewatripont (2004), or Salanie (2005).

delegation allows the firm's owners to contractually induce the manager to compete more aggressively by selling more units in the product market than would be sold absent delegation.[2] However, a potential unintended consequence of providing managers with such incentives, and one that has received little attention in the literature, is that in providing such incentives, the firm's owners may also alter their manager's incentives to voluntarily disclose her private information. Such potential "spillover" effects indicate a need to analyze *joint* delegation rather than the delegation of each activity separately. We study these spillover effects by developing a model of joint delegation of production and disclosure choices, and examine the effects of product market competition on the contract governing the delegation of those choices.

To accomplish this objective, we develop and analyze a model that synthesizes models of delegation of production choices, such as Brander and Lewis (1986) and Fershtman and Judd (1987), and models of disclosure, such as Gal-Or (1985, 1986) and Darrough (1993). Specifically, owners of each of two firms hire a manager and delegate two sequential decisions to her: an *ex ante* disclosure decision regarding whether to adopt a policy of disclosing any private information she subsequently acquires, and a product output decision that determines how successfully the firm will compete with its rival in their product market. As is typical in the literature, these decisions are influenced by the incentives the firm's owners offer their manager. Further, we follow the delegation literature by assuming that the manager's compensation depends on the firm's realized profits and revenues.

Our analysis provides two new, key insights. First, we show how delegation of disclosure, in addition to delegation of production choices, alters the contractual incentives chosen by the firms' owners. Specifically, we show that each firm's owners substitute the decision to disclose by the manager of *either* firm for direct contractual incentives to compete more aggressively by selling more units of output for any output choice by the rival firm. That is, anticipating disclosure by *either* manager motivates the owners of both firms to reduce the incentive weights they place on sales. Intuitively, each firm's owners choose how sensitive managerial compensation is to their manager's production choice, and because this sensitivity is naturally greater if the manager or her rival opt to disclose, each firm's owners can and do substitute disclosure for direct incentives. We also show that owners of the firm with a larger product market or with lower expected costs will choose incentive weights that induce their manager to compete relatively more aggressively in the product market. Further, the relative incentive weights chosen by the two firms' owners are less different when a manager is also induced by the incentive contract to adopt a policy of disclosing her private information. Conversely, owners of firms with greater cost uncertainty, such as younger firms or firms whose input prices are highly variable, will choose incentive weights that induce their manager to compete less aggressively.

Second, we show that joint delegation alters the competing firms' information environments. Specifically, owners who choose incentive weights that induce their manager to compete more aggressively simultaneously provide their manager with incentives to disclose information that is more useful in understanding her rival's costs than her own relative to the disclosure choice had the owners not delegated the choice.[3] Conversely, owners of the rival firm give their manager incentives to provide information that is more useful in understanding her own firm's costs. Thus, joint delegation shifts the information environment toward containing more information about the firm with the smaller product market or larger expected marginal costs, and away from information

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[2] Classic articles that show this include Brander and Lewis (1986), Fershtman and Judd (1987), Sklivas (1987), Katz (1991), and Hermalin (1994). Gal-Or (1997) provides an excellent survey of this part of the delegation literature.

[3] In our model, a manager's private information provides information about both the firm's and the rival firm's production costs. Our parameterization of how production costs depend on the managers' private information allows us to vary how much information about production costs becomes available to either manager when she learns her private information.

about the firm with the larger product market or smaller expected marginal costs relative to the information environment produced absent delegation of the disclosure choice. Another effect of joint delegation is that the manager who is given incentives to compete more aggressively is more likely to adopt a policy of disclosure. Thus, joint delegation shifts both the likelihood of disclosure and the content toward disclosure of information about the less aggressive competitor.

Our analysis also reveals a number of cross-sectional empirical implications associated with the type of joint delegation that we study. First, it suggests that firms that seek a competitive advantage through a cost leadership strategy and/or focus on mass markets by selling less differentiated products offer managers contracts that rely more heavily on revenue than profits relative to firms that adopt differentiation strategies and/or focus on niche markets.[4] Further, disclosures of the former set of firms will differ from disclosures of the latter set of firms in two ways. First, the latter firms' managers will offer fewer disclosures, and their disclosures will be more focused on their own product costs than on their rival's product costs. Second, regardless of whether a firm employs a cost leadership or differentiation strategy, the owners reduce the incentive weight on revenues when their manager offers more frequent voluntary disclosures such that disclosure substitutes for incentives. Third, our analysis suggests that younger firms or those with greater cost uncertainty are expected to offer managers contracts that rely less heavily on revenue than mature firms or those with relatively stable production costs. However, both types reduce the incentive weight on revenues when the manager is motivated to adopt a policy of providing voluntary disclosures. Fourth, our analysis suggests that there are predictable cross-sectional differences in the information environments of competing firms. Specifically, more information is disclosed about the cost structure of firms with smaller expected profit margins, smaller expected market shares, or more cost uncertainty. Conversely, less information is provided about firms with greater expected profit margins, greater expected market shares, or less cost uncertainty. Finally, our analysis reveals a link between the disclosure policy choices of a firm's rivals and the incentive contract offered to the firm's manager. Specifically, owners place greater incentive weight on revenues when rival managers are not expected to offer voluntary disclosures. This means that a firm's owners motivate their manager to compete more aggressively in the product market and increase the likelihood that their manager provides voluntary disclosures when their firm's rivals are less likely to offer their own voluntary disclosures.

The remainder of the article is organized as follows. Section II discusses the related literature and Section III contains a description of our model. Sections IV, V, and VI describe equilibrium in the final, second, and first stages of the game, respectively. We describe our results on delegation of operating and disclosure choices in Section VII, and conclude in Section VIII.

II. RELATED LITERATURE

Our model, which synthesizes models of delegated production choice and of *ex ante* voluntary disclosure, is related to two distinct literatures.[5] The literature on delegated production choice is based on an insight in Schelling (1960), and early formal models include Brander and Lewis (1986), Fershtman and Judd (1987), Sklivas (1987), and Hermalin (1994). All demonstrate that owners can benefit financially from contractually delegating to a manager control of their firm's strategic response to its rivals. Contractual delegation of strategic decision-making serves as a precommitment device allowing owners of firms an opportunity to acquire a competitive advantage

[4] Here, we use Porter's (1980) contrast of cost leadership versus differentiation or niche business strategies to help link our theoretical constructs to observed business strategies.

[5] In work that complements ours, Yoon (2012) introduces delegation and relative performance evaluation into a model of *ex post* disclosure and finds that delegation combined with relative performance evaluation (RPE) offers an alternative solution to the well-known "unraveling" result.

over their rivals in their product markets. Katz (1991) suggests that these results require that managers' contracts be observable. However, recent work by Kockesen and Ok (2004) and Kockesen (2007) clarifies this issue by showing that the benefits from delegation are still essentially present even in the absence of observable contracts.[6] In an important extension, Hughes and Williams (2008) examine owners' incentives to *voluntarily* disclose the contractual arrangements with their managers. Their key result is that firms voluntarily disclose contractual arrangements if the costs of disclosure are small enough. They also show that firms disclose if the non-strategic benefits of such commitments are large or if owners have access to enough different commitment mechanisms. Our work contributes to this literature by showing how delegation of both disclosure and strategic production choices alters the contractual incentives chosen by the firm's owners.

Such key articles in the literature on *ex ante* disclosure as Gal-Or (1985, 1986), Darrough (1993), Sankar (1995), and Raith (1996) abstract from the effects of delegation by assuming that the incentives of the managers and owners are perfectly aligned. They focus on the firm's willingness to disclose private information about its own production costs, referred to as the private or independent values case, or market information that affects the payoffs of both firms equally, referred to as the common values case.[7] The central result in this literature is that the firms' decisions to disclose private information depend on whether the firms choose quantities or prices when they compete in their product market and whether the information structure examined is the private or common values structure.[8] However, the key insight is that the decision to disclose depends on how disclosure affects the firms' product market rivalry. This suggests that delegation of production choices will also affect the manager's disclosure decision and *vice versa*. We contribute to the *ex ante* disclosure literature by analyzing the effects of joint delegation of production and disclosure choices and how the owners' choices of contractual incentives are affected by the anticipated delegated production and disclosure choices.

Our model is most closely related to Theilen (2007), who considers a homogeneous product market with private information about costs (the private values case). He shows that if the firms' owners delegate *only* the production decision to their manager, but retain the disclosure decision, then delegation may affect the owners' equilibrium disclosure choices. Specifically, if the managers' pay could be reduced when revenues increase, then it is possible to reverse the disclosure result in Gal-Or (1985) and Darrough (1993). If not, then the disclosure policy choice is unaffected by delegation. Our model differs from Theilen's (2007) in that we examine joint delegation, permit the managers of firms to sell heterogeneous products, and do not restrict attention to the private values case. Our results differ from his because we develop conditions under which delegation of production decisions affects disclosure policy choices, and by analyzing how delegation of disclosure choice affects contractual incentives to motivate production choices.

Delegation and disclosure have also been studied in a single principal-agent setting. This literature is extensive, but key articles that focus on delegated disclosure include Dye (1985), Arya, Glover, and Sunder (1998), and Gigler and Hemmer (2001). Dye (1985) shows that managers may

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[6] A strand of the literature considers the effects of allowing observable contracts that depend on the profits earned by all of the rivals in the industry. The key result in this literature is that collusive outcomes can be supported with such contracts. See Fershtman, Judd, and Kalai (1991), Fershtman and Kalai (1997), Hughes and Kao (1997), Kockesen and Ok (2004), Katz (2006), Kockesen (2007), or Manasakis, Mitrokostas, and Petrakis (2010), among others.

[7] Raith (1996) offers an interesting generalization of Gal-Or (1985, 1986) and Darrough (1993) by introducing imperfect signals. He shows that equilibrium disclosure policies depend on the quality of the signal a firm receives and on the correlation of their signals. See, also, the nice summary of this literature in Vives (2008).

[8] There is an extensive literature that examines *ex ante* disclosure by firms competing in product markets. Contributions include Maleug and Tsutsui (1996), who focus on disclosure of information about the slope of the market demand curve; Arya and Mittendorf (2007), who examine the impact of third-party information on *ex ante* disclosure policy; Currarini and Feri (2007), who focus on disclosure by subsets of rivals; and Bagnoli and Watts (2014), who study the firms' information acquisition decisions and how they are affected by their subsequent disclosure policy adoption decisions.

not find it optimal to voluntarily disclose information about their performance measure. Arya et al. (1998) show that owners may find it useful to restrict disclosure of interim performance measures, and Gigler and Hemmer (2001) establish that an owner who chooses a more conservative accounting system will induce less timely voluntary disclosures. Our analysis complements these articles by focusing on the joint delegation of disclosure and production choices and on the interactions associated with joint delegation of these choices by rivals.

III. MODEL

We develop and analyze a model that allows us to examine spillover effects associated with joint delegation of production and disclosure choices. In our model, the firm's owners contractually delegate both decisions to the firm's manager. The manager then chooses whether to commit to adopt a policy of disclosing her private information prior to learning it. Following that decision, the manager learns her private information, follows her chosen disclosure strategy, observes any information her rival discloses, and then competes with the rival firm in the product market by choosing output.

In the first stage of the game, owners simultaneously delegate future production and disclosure choices to their manager. We follow the analysis in Fershtman and Judd (1987) and Sklivas (1987) and assume that the owners of a firm motivate their manager's decisions by paying her based on the firm's realized profits and realized revenues. Similar results are obtained if the manager is paid based on the firm's realized profits and cost reduction efforts instead.

In particular, we assume that the manager's compensation is proportional to:

$$P_i = \pi_i + \lambda_i S_i \quad i = 1, 2$$

where π_i is firm i's realized profits and S_i is its realized sales, and the owners of each firm select $\lambda_i \geq 0$ to maximize their expected profits.

In the second stage, given the incentive weights chosen by the firms' owners, each manager selects her firm's *ex ante* disclosure policy. Let $d_i \in \{D, N\}$ be the disclosure policy chosen, where D represents a policy of disclosure and N a policy of nondisclosure. As is standard, we assume that each manager makes the disclosure choice without knowing her rival's choice. The managers then learn their private information and follow their chosen disclosure policies. Consequently, each manager potentially has two sources of information: her private information and, if her rival adopts a policy of disclosure, the private information disclosed by her rival manager. We assume that if a disclosure is made, it is truthful.

In the third and final stage of the game, after learning all of the available information, the firms engage in Cournot competition in their product markets by having their managers choose how much of their product to sell. We assume that the risk-neutral managers make their *ex ante* disclosure choices and their output decisions to maximize their own expected payoffs. Everything else is common knowledge.

The above structure, except for the delegation of the disclosure decision, is often used in the *ex ante* disclosure literature (e.g., Gal-Or 1985, 1986; Darrough 1993; Sankar 1995; Raith 1996). However, we alter the information structure to facilitate our study of the impact of a manager's information on the delegation decisions of the owners. To simplify the analysis, we fix what the manager knows, but parameterize the firms' cost functions. This allows us to alter the importance of that information on the firm's profits and on the rival firm's profits, and thereby study the impact of a manager's private information on the delegation decisions.

Each firm's demand curve is:

$$p_i = \alpha_i - q_i - tq_j \quad i = 1, 2 \tag{1}$$

where α_i is the intercept of firm i's demand curve, q_i is the amount of product i that firm i sells, q_j is the amount of product j that firm j sells, and $t \in (0, 1)$ to ensure that the firms sell substitute

products. Both managers know both firms' demand curves, but have private information about their own firm's cost structure. In particular, we assume manager i knows c_i, and we parameterize each firm's marginal costs of production, k_i, as:

$$k_i = \delta_{ii}c_i + \delta_{ij}c_j$$

with $\delta_{ij} \in [0, 1]$, $i = 1, 2$, $j \neq i$. There are no fixed costs of production.

There are two advantages of adopting this information structure. First, the parameters δ_{ii} and δ_{ij} create a flexible information structure for the managers' private information in which the private and common values structures previously analyzed in the *ex ante* disclosure literature are special cases. In particular, the private values information structure is represented by $\delta_{11} = \delta_{22} = 1$ and $\delta_{12} = \delta_{21} = 0$; and the common values information structure by $\delta_{11} = \delta_{22} = \delta_{12} = \delta_{21} = 1$. Second, it offers a parsimonious way to represent information structures in which a manager's private information is differentially useful to the two firms. For example, large values of δ_{ii} and δ_{ji} represent a situation in which manager i's private information, c_i, is very useful to both firms because both firms' marginal costs are sensitive to the realization of c_i. This type of situation can arise if both firms are heavily dependent on a particular input to their production processes and c_i represents manager i having private information about the input's price. Alternatively, large values of δ_{ii} and δ_{ij} relative to δ_{jj} and δ_{ji}, respectively, represent a situation in which both managers' private information is more useful to firm i than to firm j because firm i's costs are particularly sensitive to the realizations of c_i and c_j. This type of situation can arise if the firms sell differentiated products and there is more uncertainty about the costs of making one version of the product. Thus, our structure allows us to examine how contractually delegating joint disclosure and production decisions depends on the relative importance of the manager's private information about the two firms' cost functions.

To complete the description of the information structure, we assume that the firms' owners and managers have common priors such that all variables are drawn from known distributions with finite means and variances. We also assume that c_1 and c_2 are independent so that the distribution of c_j given c_i is independent of c_i for $i = 1, 2$; $j \neq i$. To ensure interior solutions in the final stage of the game, each firm's demand function is such that the value of the intercept parameter for each firm exceeds the largest value of that firm's marginal costs. We seek a perfect Bayes-Nash equilibrium of the model and begin by solving the final stage of the game, in which the firms compete in the product market.

There are three features of the model that deserve additional discussion. The first feature is that owners offer their manager a contract that depends on both firm profits and revenues. As we noted above, owners delegate production choices and reward the manager based on revenues and profits as a means of precommitting to sell more for any output choice by the firm's rival than would be optimal absent delegation and precommitment. The owners adopt such a contract to specifically change the marginal benefit/marginal cost calculation of their manager to motivate her to produce more output. Institutionally, corporate executive contracts often use revenues as a performance metric. For example, Huang, Marquardt, and Zhang (2014) report that 30 percent of the firms in their sample *explicitly* use revenue as a performance metric for CEO compensation, and the Grant Thornton (2013) *Financial Executive Compensation Survey* indicates that virtually all financial executive contracts use revenue as a performance metric.[9]

The second feature is our assumption that the incentive contracts chosen by the firm's owners are observed by all parties, which, at one point, was a controversial assumption. Specifically, Katz (1991) suggested that the main results in the delegated production choice literature were the result of this observability assumption. However, subsequent work (Kockesen and Ok 2004; Kockesen

[9] Interestingly, IBM's CEO, Ginni Rometty, announced that following IBM's 2013 revenue miss, she and other top IBM executives would not receive their 2013 bonuses (Bort 2014).

2007) clarified the issue by showing that the benefits from delegation remain even in the absence of observable contracts. We choose to assume observability of the contracts for two reasons. First, the assumption simplifies the analysis without changing the qualitative nature of the results, as shown by Kockesen and Ok (2004) and Kockesen (2007). Second, observability is consistent with the current regulatory environment, as surveyed by Larcker and Tayan (2012). Specifically, disclosure is required by the Securities and Exchange Commission's (SEC) (2006) decision to require "plain English" discussion of the key components of CEO compensation within each firm's 10-K filing. This requirement has been reiterated in the SEC (2007) staff review compliance and the SEC (2009) update provided in a speech by Shelley Parratt, Deputy Director, Division of Corporation Finance, SEC. In addition, the 2010 Dodd-Frank legislation enhances disclosure requirements for pay-for-performance compensation (see SEC 2013). Together, these regulatory requirements suggest that contract observability is a reasonable approximation.

The third feature that deserves additional discussion is our focus on *ex ante* precommitment to disclose or not. There are two concerns with *ex ante* disclosure. First, will the decision-maker prefer to commit to the disclosure decision? Second, what forces keep the decision-maker from reneging on the commitment? Turning to the first concern, it is straightforward to show that the manager's payoff is greater if she chooses *ex ante* rather than *ex post* disclosure, because both the firm's profits and the manager's payoff are convex in the amount the firm sells in its product market. Because the decision to choose an *ex ante* or an *ex post* disclosure strategy must be made prior to the manager learning her private information, the optimal choice is the one that maximizes expected profits, which is equivalent to the choice that produces the greater variance in the subsequent output decision. If the *ex ante* disclosure decision is to commit to disclose, then the variance of output given disclosure exceeds the variance given no disclosure. On the other hand, if the *ex ante* decision is to commit to not disclose, then the variance of output given no disclosure exceeds the variance given disclosure. Because *ex post* disclosure results in disclosure for some realizations of the private information and no disclosure for the remaining realizations, the *ex ante* variance of output is a weighted average of the variance with disclosure of all realizations and the variance with disclosure of none. Thus, the manager prefers to adopt an *ex ante* rather than an *ex post* disclosure strategy. The other concern with *ex ante* disclosure, noted above, is the possibility of reneging on the commitment. In our setting, this is less of a concern because the owners delegate the disclosure choice to the manager. Thus, owners, who prefer *ex ante* disclosure, can achieve this outcome by imposing sufficient penalties on the manager to ensure that the benefits from reneging never exceed this penalty. There are always benefits to reneging for some realizations of the manager's private information, but our assumptions of finite supports for the random variables ensure that these benefits are bounded. Interestingly, delegation of disclosure plays the same role as delegation of production decisions by allowing the owners to "tie" their hands and precommit to subsequent choices that they would renege on otherwise.

IV. FINAL STAGE: DELEGATED PRODUCT MARKET COMPETITION

We begin by determining each manager's equilibrium output choice for every possible choice of incentive weights by the firm's owners and every possible disclosure choice by each of the managers. When firms compete in their product markets, each manager chooses the output level that maximizes her expected payoff conditional on all information available to her and all prior decisions. Thus, her decision depends on her own private information, c_i, the disclosure strategy chosen by her rival, d_j, the information the rival's strategy requires disclosing, and her own disclosure strategy, d_i.

The manager of firm i solves: $\max_{q_i} E[\pi_i + \lambda_i S_i | \phi_i]$, where ϕ_i represents the information manager i has about c_i and c_j at the time of her production choice. Rewriting:

$$\max_{q_i} E\left[(1+\lambda_i)(\alpha_i - q_i - tq_j)q_i - (\delta_{ii}c_i + \delta_{ij}c_j)q_i|\phi_i\right].$$

The first-order condition is:

$$q_i = E\left[\frac{1}{2}(\alpha_i - tq_j) - \frac{1}{2(1+\lambda_i)}(\delta_{ii}c_i + \delta_{ij}c_j)|\phi_i\right].$$

The usual calculations yield equilibrium quantity choices and payoffs to the managers.

Proposition 1: When both managers have private information about costs, Cournot equilibrium quantities for each disclosure combination are:

$$q_i(D,D) = \frac{1}{(4-t^2)}\left(2\alpha_i - t\alpha_j - \frac{2k_i}{1+\lambda_i} + \frac{tk_j}{1+\lambda_j}\right)$$

$$q_i(D,N) = \frac{1}{(4-t^2)}\left(2\alpha_i - t\alpha_j - \frac{2E[k_i|c_i]}{1+\lambda_i} + \frac{tE[k_j|c_i]}{1+\lambda_j}\right)$$

$$q_i(N,D) = \frac{1}{(4-t^2)}\left(2\alpha_i - t\alpha_j + \frac{t^2E[k_i|c_j]}{1+\lambda_i} + \frac{tE[k_j|c_i]}{1+\lambda_j}\right) - \frac{k_i}{1+\lambda_i}$$

$$q_i(N,N) = \frac{1}{2(4-t^2)}\left(4\alpha_i - 2t\alpha_j - \frac{(4-t^2)E[k_i|c_i]}{1+\lambda_i} - \frac{t^2E[k_i]}{1+\lambda_i} + \frac{2tE[k_j]}{1+\lambda_j}\right).$$

where $E[k_i|c_i] \equiv \delta_{ii}c_i + \delta_{ij}E[c_j]$ and $E[k_i] \equiv \delta_{ii}E[c_i] + \delta_{ij}E[c_j]$. Equilibrium payoffs to the managers in this stage of the game are proportional to $P_i(d_i, d_j) = (1+\lambda_i)[q_i(d_i, d_j)]^2$ for $i = 1, 2; j \neq i$, and equilibrium profits for the owners are $\pi_i = [q_i(d_i, d_j)]^2$ for $i = 1, 2; j \neq i$.

The differences in equilibrium quantity choices arise from the managers' prior disclosure choices and the owners' prior contracting choices. When both managers choose to disclose their private information, both are fully informed about each firm's production costs leading to the standard full-information Cournot solution. When one manager chooses to disclose and the other chooses not to, the latter is fully informed, but the former must use her priors concerning her rival's private information when estimating both firms' costs of production. If neither manager chooses to disclose, then each knows only their private information about costs, as well as both firms' customer demands.

Before discussing a key implication of Proposition 1, we observe that if we specialize our information structure to the private values case where $\delta_{ii} = \delta_{jj} = 1$; $\delta_{ji} = \delta_{ij} = 0$ or the common values case where $\delta_{ii} = \delta_{ji} = \delta_{ij} = \delta_{jj} = 1$ and eliminate the impact of delegation by setting $\lambda_i = \lambda_j = 0$, the equilibrium strategies simplify to those obtained in Gal-Or (1985, 1986) or Darrough (1993), showing that our information structure is an appropriate generalization of the prior literature.

The main result in Proposition 1 is that in equilibrium, the manager's payoff is proportional to $P_i(d_i, d_j) = (1+\lambda_i)[q_i(d_i, d_j)]^2$, which is convex in the manager's output choice. The reason that convexity is important is that when we examine equilibrium disclosure choices, these choices will be made prior to the manager learning her private information. As a result, managers will seek to maximize their expected payoffs, and our convexity result will mean that they seek to maximize the variance of their output decisions. In other words, the managers will seek the option that makes their subsequent output choices most sensitive to the realization of their private information.

Proposition 1 also offers two insights regarding a manager's equilibrium quantity choice. First, the equilibrium quantities illustrate the effect of strategic delegation, as originally described in Fershtman and Judd (1987) and Sklivas (1987). Firm i's equilibrium output, $q_i(d_i, d_j)$, is increasing in λ_i and decreasing in λ_j for every pair of disclosure decisions (d_i, d_j). Specifically:

$$(1): \frac{\partial q_i(D,D)}{\partial \lambda_i} = \frac{2k_i}{(4-t^2)(1+\lambda_i)^2} > 0 \text{ and } \frac{\partial q_i(D,D)}{\partial \lambda_j} = \frac{-tk_j}{(4-t^2)(1+\lambda_j)^2} < 0;$$

$$(2): \frac{\partial q_i(D,N)}{\partial \lambda_i} = \frac{2E[k_i|c_i]}{(4-t^2)(1+\lambda_i)^2} > 0 \text{ and } \frac{\partial q_i(D,N)}{\partial \lambda_j} = \frac{-tE[k_j|c_j]}{(4-t^2)(1+\lambda_j)^2} < 0;$$

$$(3): \frac{\partial q_i(N,D)}{\partial \lambda_i} = \frac{-[-(4-t^2)k_i + t^2E[k_i|c_i]]}{(4-t^2)(1+\lambda_i)^2} > 0,$$

which is positive because, in equilibrium:

$$-(4-t^2)k_i + t^2E[k_i|c_i] < 0 \text{ and } \frac{\partial q_i(N,D)}{\partial \lambda_j} = \frac{-tE[k_j|c_j]}{(4-t^2)(1+\lambda_j)^2} < 0; \text{ and}$$

$$(4): \frac{\partial q_i(N,N)}{\partial \lambda_i} = \frac{2E[k_i|c_i]}{(4-t^2)(1+\lambda_i)^2} > 0 \text{ and } \frac{\partial q_i(N,N)}{\partial \lambda_j} = \frac{-tE[k_j|c_j]}{(4-t^2)(1+\lambda_j)^2} < 0.$$

To see why, assume that only the owners of firm i motivate their manager by placing incentive weight on sales. That is, they choose $\lambda_i > 0$ while the rival owners choose $\lambda_j = 0$. These incentives cause the manager of firm i to sell more than a profit-maximizing manager for every level of output by the rival. The rival manager, anticipating the effect of the incentives, finds it optimal to "accommodate" them by reducing the output she chooses. As a result, firm i's sales are larger and firm j's sales are smaller than they would be absent delegation when $\lambda_i = \lambda_j = 0$. Further, even though total output is greater than if both managers were simply maximizing profits, sufficient sales are shifted to firm i so that its profits actually increase. In essence, delegation is a means of precommitting the firm to produce greater output, which shifts profits toward the firm. In equilibrium, rivalry causes both firms' owners to provide incentives that emphasize revenues, as well as profits, in order to motivate each manager to choose a larger output.

Second, the equilibrium quantities reveal the importance of parameterizing the usefulness of a manager's private information. Recall that the usefulness of the manager and her rival knowing c_i depends on the values of δ_{ii} and δ_{ji}. Holding all else constant, in equilibrium for a given realization of c_i, larger values of δ_{ii} increase the firm's marginal costs of production, $k_i = \delta_{ii}c_i + \delta_{ij}c_j$. Because the firm's equilibrium output choice, $q_i(d_i, d_j)$, is decreasing in k_i or $E[k_i|c_i]$, an increase in δ_{ii} will result in the manager choosing a smaller output for any realization of her private information. Similarly, holding all else constant, larger values of δ_{ji} increase $k_j = \delta_{ji}c_i + \delta_{jj}c_j$. Because firm i's equilibrium output choice, $q_i(d_i, d_j)$, is increasing in k_j or $E[k_j|c_j]$, an increase in δ_{ji} will result in the manager of firm i choosing a larger output for any realization of her private information.[10] Below, we show that these features of the equilibrium quantity choices will have important effects on the

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[10] This effect is the result of the firms being Cournot competitors. Under Cournot competition, the managers' output choices are strategic substitutes, and that feature generates the δ_{ii} and δ_{ji} terms having opposite signs in Proposition 1. Had the firms been Bertrand competitors, their price choices would be strategic complements and the terms would have the same signs.

managers' disclosure choices and, therefore, highlight the importance of analyzing our more general information structure.

Finally, we observe that the effects of delegation described above appear to work by implicitly lowering the manager's perceived cost of selling additional units of output even though the owners are using revenue-based incentives. This feature is due to the linearity of both the incentive contract and the firm's demand, which allow revenue incentives to operate "as if" the manager faces reduced marginal costs of additional output.

Having determined equilibrium quantities chosen in the final stage of the game, we turn to the second stage and examine under what conditions the managers choose to commit to disclose.

V. STAGE 2: DELEGATED DISCLOSURE CHOICES

The equilibrium quantities and payoffs described in Proposition 1 are conditional on each possible specification of both the managers' private information and their disclosure policy adoption decisions. To determine the managers' equilibrium disclosure policy decisions for all possible choices of incentive weights by the firms' owners, we turn to an analysis of their *ex ante* disclosure incentives. Proposition 1 provides the information needed to describe the Bayes-Nash equilibrium of the second stage of our game as the equilibrium of the associated normal form game whose payoffs are the expected profits computed for each decision:

		Firm 2	
		D	N
Firm 1	D	$E[P_1(D, D)]$, $E[P_2(D, D)]$	$E[P_1(D, N)]$, $E[P_2(N, D)]$
	N	$E[P_1(N, D)]$, $E[P_2(D, N)]$	$E[P_1(N, N)]$, $E[P_2(N, N)]$

Given this, the main result of our analysis on delegated disclosure is presented in Theorem 1 and illustrated in Figure 1. All proofs are in Appendix A.

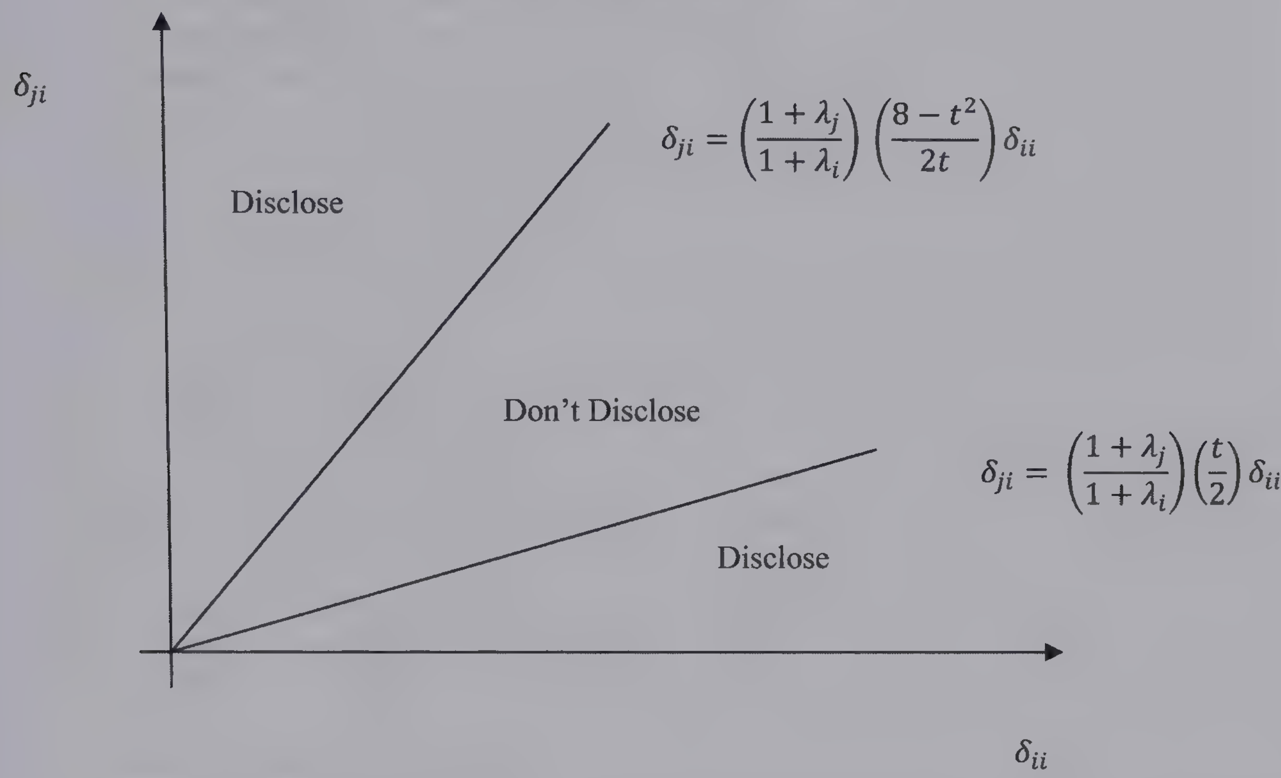
Theorem 1: When the firms are Cournot competitors, for manager i ($i = 1, 2$; $i \neq j$), disclosure of cost information is a dominant strategy if (C1) is satisfied ($d_i^* = D$), and no disclosure is a dominant strategy if (C1) is not satisfied ($d_i^* = N$), where:

$$\left(\frac{1+\lambda_i}{1+\lambda_j}\right)\left(\frac{2}{t}\right)\delta_{ji} < \delta_{ii} \;\; or \;\; \left(\frac{1+\lambda_i}{1+\lambda_j}\right)\left(\frac{2t}{8-t^2}\right)\delta_{ji} > \delta_{ii}. \tag{C1}$$

Theorem 1 offers insight into when a manager chooses to adopt a policy of disclosing her private information and when she chooses to adopt a policy of not disclosing her private information once she learns it. (C1) indicates that the manager chooses to disclose if her private information, c_i, is sufficiently differentially informative about her firm's marginal costs relative to her rival's marginal costs. A value of δ_{ii} large enough that the first inequality in (C1) is satisfied means that firm i's marginal costs, k_i, are more sensitive to the realization of the manager's private information, c_i, and so variations in its realized value translate into larger variations in firm i's marginal costs. As a result, the manager's private information is more informative about her own firm's marginal costs. Instead, when δ_{ji} is large so that the second inequality in (C1) is satisfied, then firm j's marginal costs, k_j, are more sensitive to the realization of the manager's private information, c_i. In this case, variations in its realized value translate into larger variations in firm j's marginal costs, making the manager's private information more informative about her rival's marginal costs.

Intuitively, a manager benefits from increases in the variability of the equilibrium outputs chosen by either manager as a function of their private information, because each manager's

FIGURE 1
Equilibrium Disclosure Choice



This figure provides a graphical representation of condition (C1) from Theorem 1. Specifically, $\delta_{ji} = \left(\frac{1+\lambda_j}{1+\lambda_i}\right)\left(\frac{t}{2}\right)\delta_{ii}$ represents the first part of (C1) and $\delta_{ji} = \left(\frac{1+\lambda_j}{1+\lambda_i}\right)\left(\frac{8-t^2}{2t}\right)\delta_{ii}$ represents the second part.

payoff, as described in Proposition 1, is convex in the manager's output choice. Thus, the manager's benefits from choosing not to disclose her private information arise only from her ability to tailor her output decision to her private information. If, instead, she chooses to disclose her private information, then the rival manager can also tailor her output to the information revealed by the disclosure, which we call the information effect, and strategically adjust her output more effectively to the disclosing firm's output choice, which we call the strategic effect. Because the firms' output choices are strategic substitutes, increases in the sensitivity of the rival's output to the disclosing manager's private information dampen the variability of the disclosing manager's output choice. This is the disadvantage to disclosure. As a result, with Cournot competition in the product market, the manager only chooses to disclose when her private information is sufficiently asymmetrically useful to her and her rival. If the information is predominantly useful to the disclosing manager, then the information effect is small and disclosure is preferred. Similarly, if the information is predominantly useful to the rival manager, then the information effect is large, increasing the benefits of disclosure, while the benefits from not disclosing are small.[11]

[11] This is the key difference between our results when firms are Cournot versus Bertrand competitors. When firms compete in their product markets by choosing prices, prices are strategic complements rather than strategic substitutes (Bulow, Geanakoplos, and Klemperer 1985), and so increased variability in one firm's price amplifies the variability in the rival's price. With Cournot competition, increased variability in one firm's output dampens the variability in the rival's output. As a result, with Bertrand competition, the managers choose to disclose information that is useful to both firms and to not disclose information that is sufficiently asymmetrically useful to the two firms.

Theorem 1 highlights some important effects of delegating both disclosure and production choices to a manager. Specifically, the disclosure decisions made by the firms' managers are almost surely different from the profit-maximizing disclosure choices that would arise if $\lambda_1 = \lambda_2 = 0$. Thus, as we conjectured, delegating both production and disclosure decisions leads to changes in the firms' disclosure choices. In particular, (C1) shows that the impact of delegating production choices on the disclosure decision depends on whether the ratio $(1+\lambda_i)/(1+\lambda_j)$ exceeds 1 or not. If it exceeds 1, then the owners of firm i have chosen to motivate their manager to compete more aggressively, relative to their rival's manager, in the product market by selling more units than the profit-maximizing quantity. These incentives also shift their manager's disclosure away from disclosing private information that is more informative about her own firm's costs and toward disclosing information that is more informative about the rival's costs.[12] Conversely, if the ratio is less than 1, then owners of firm i have chosen to motivate their manager to compete less aggressively in the product market, and these incentives shift their manager's disclosure toward disclosing private information that is more informative about her own firm's costs and away from disclosing information that is more informative about the rival's costs. Thus, joint delegation shifts the information environment toward having more information about the cost structure of the less aggressive firm and less information about the cost structure of the more aggressive firm. To simplify the discussion in this section, we will refer to the firm whose owners have chosen a larger incentive weight on revenues as the more aggressive firm because such incentive weights motivate the manager to choose to sell more units in the product market for any output choice of her rival.

Interestingly, in addition to shifting the information disclosed, joint delegation also changes the likelihoods that the firms' managers choose to disclose their private information. In particular, joint delegation increases the likelihood of disclosure by the manager of the more aggressive competitor and decreases the likelihood of disclosure by the manager of the less aggressive competitor. To make this precise, recall that $(\delta_{ii}, \delta_{ji}) \in [0, 1] \times [0, 1]$, and assume a uniform distribution with full support on the unit square. In this case, the probability of disclosure is greater when the area associated with disclosure is greater. Theorem 1 indicates that the area of the unit square representing pairs of δ_{ii}, δ_{ji} that lead the manager to adopt a policy of disclosure is greater than it is when the disclosure choice is made by the owners directly, when $\lambda_i = 0$. To see why, note that for each δ_{ii}, an increase in λ_i shrinks the set of δ_{ji} that satisfies the first part of (C1), $\delta_{ji} < \frac{t(1+\lambda_j)}{2(1+\lambda_i)}\delta_{ii}$, but expands the set of δ_{ji} that satisfies the second part of (C1), $\delta_{ji} > \frac{(1+\lambda_j)(8-t^2)}{2t(1+\lambda_i)}\delta_{ii}$. Beginning from the situation when $\lambda_i = \lambda_j$, the net effect of an increase in λ_i is to increase the set of δ_{ji} for each δ_{ii} that leads to disclosure by the manager of the more aggressive firm because $\frac{8-t^2}{2t} > \frac{t}{2}$. Summarizing, joint delegation increases the probability of disclosure by the more aggressive firm and shifts the disclosures by that firm's manager toward disclosing information that is more informative about the rival and away from disclosing information about her own firm's costs. The reverse is true for the less aggressive firm in that the probability of disclosure declines, but disclosures are shifted toward information about the manager's firm's costs and away from information about the rival.[13]

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[12] We use the term shift in this context to indicate that the manager's disclosure choice has been altered relative to her choice if $\lambda = 0$, when the manager is not offered incentives to increase output relative to the profit-maximizing quantity.

[13] This can be shown by replacing the increase in λ_i with a decrease in λ_i in the explanation for the more aggressive competitor. Specifically, a decrease in λ_i expands the set of δ_{ji} that satisfy the first part of (C1), $\delta_{ji} < \frac{t(1+\lambda_j)}{2(1+\lambda_i)}\delta_{ii}$, but shrinks the set of δ_{ji} that satisfy the second part of (C1), $\delta_{ji} > \frac{(1+\lambda_j)(8-t^2)}{2t(1+\lambda_i)}\delta_{ii}$. Because $\frac{(8-t^2)}{2t} > \frac{t}{2}$, beginning from the situation when $\lambda_i = \lambda_j$, the net effect of a decrease in λ_i is to decrease the set of δ_{ji} for each δ_{ii} that leads to disclosure by the manager for the more aggressive competitor.

Intuitively, the shift in the information environment introduced by joint delegation relative to the no-delegation outcome leads to more information being disclosed about the less aggressive firm in equilibrium. This happens because owners who motivate their manager to be the more aggressive competitor increase the benefits to that manager of tailoring her output choice to her private information. As a result, the benefits to the manager and owners of the firm from not disclosing information that is particularly useful to the disclosing manager increase relative to the benefits from disclosing the information. This reduces the manager's incentive to disclose information about her firm's costs and increases her incentives to disclose information predominantly useful to her rival. Thus, joint delegation alters the information environment by reducing the information disclosed about the more aggressive firm's costs and increasing the information disclosed about the less aggressive firm's costs.

Unlike the prior *ex ante* disclosure literature, such as Gal-Or (1985, 1986) and Darrough (1993), an implication of Theorem 1 is that disclosure choices also depend on the extent to which the firms' products are differentiated. This is most easily seen by noting that the value of t, which is our measure of how similar the firms' products are, helps determine whether either inequality in (C1) is satisfied. Examining those inequalities shows that with joint delegation, the effects on the disclosure choices by the firms' managers are greater if the firms' products are less differentiated. Specifically, (C1) tells us that if the firms sell more homogenous products, then the manager motivated to be the more aggressive competitor is more likely to choose to disclose her private information, and the information disclosed is more heavily shifted toward disclosure of information useful in understanding her rival's costs.[14] The same arguments apply for the manager motivated to be the less aggressive competitor. However, in this case, all effects are larger because this manager's incentive weight is smaller than her rival's.

Finally, we can focus on the special cases of private and common values cases analyzed originally by Gal-Or (1985, 1986) and Darrough (1993). In our model, the private values information structure has $\delta_{ii}=\delta_{jj}=1$, $\delta_{ij}=\delta_{ji}=0$, and the common values information structure has $\delta_{ii}=\delta_{ji}=\delta_{ij}=\delta_{jj}=1$. The proof of Corollary 1 follows immediately from (C1) after substituting the restrictions just mentioned.

Corollary 1: (Private and Common Values Information Structures):

(a) For the common values information structure, the unique equilibrium has both managers adopting a policy of not disclosing their private information unless the incentive weights chosen by the owners of the two firms are sufficiently different. If so, then the owners placing greater emphasis on revenues induce their manager to adopt a policy of disclosure, while their rival's owners do not.

(b) For the private values information structure, the unique equilibrium has both managers adopting a policy of disclosing their private information about production costs with or without delegation.

It is straightforward to extend our analysis to the case when managers are not certain whether their information is, in fact, private. To see why, suppose that the manager believes that there is a positive probability, μ, that the cost information she will receive will be private and a positive

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[14] Specifically, if $\lambda_i > \lambda_j$, then for each fixed δ_{ii}, the first condition in (C1) describes the set of δ_{ji} associated with disclosure as those satisfying $\delta_{ji} < \frac{t(1+\lambda_j)}{2(1+\lambda_i)}\delta_{ii}$. Because the maximal value is increasing in t, this condition implies that the set of δ_{ji} for each δ_{ii} that is disclosed is greater when the firms sell more homogeneous products. Similarly, for each fixed δ_{ii}, the second condition in (C1) describes the set of δ_{ji} associated with disclosure as those satisfying $\delta_{ji} > \frac{(1+\lambda_j)(8-t^2)}{2t(1+\lambda_i)}\delta_{ii}$. Because the minimal value in decreasing in t, this condition also implies that the set of δ_{ji} for each δ_{ii} that is disclosed is greater when the firms sell more homogeneous products.

probability, $1 - \mu$, that it will not be private. If the manager were to adopt a policy of disclosure, then her payoff would be the payoff from disclosure described in Proposition 1. This follows because the manager's costs are known to the rival, either because she disclosed them or because they were not private. On the other hand, if the manager were to choose to not disclose, then her payoff would be a weighted average of her payoff in Proposition 1 when she does not disclose and her payoff when her information is not private. Thus, the differences in the payoffs are determined by the differences studied in Theorem 1 and produce the same equilibrium decisions. Said differently, when the manager knows that her information is not private, her disclosure choice is payoff-irrelevant, and so her disclosure choice is based only on the possibility that her information is private.[15]

It is also straightforward to compare the results reported in Theorem 1 to the outcome if the owners were to choose not to delegate the disclosure decision to their manager. If the disclosure choice is delegated, as in our model, then the manager makes the disclosure decision to maximize $E[P_i(d_i, d_j)] = E[(1 + \lambda_i)(q_i(d_i, d_j)^2)]$. If the disclosure decision is, instead, retained by the owners, then they would make the disclosure decision to maximize $E[\pi_i(d_i, d_j)] = E[q_i(d_i, d_j)^2]$. These objective functions are proportional, and so the decision to adopt a policy of disclosure would differ between the two cases, but be qualitatively the same with or without delegation.

VI. STAGE 1: THE OWNERS' DELEGATION DECISIONS

We now use the results in Proposition 1 and Theorem 1 to study the owners' equilibrium delegation decisions of how much emphasis to place on revenues in the manager's employment contract. Each firm's owners choose the incentive weight that maximizes expected profits, taking into account both whether the manager will subsequently find it optimal to adopt a policy of disclosure and the impact on how she competes in the firm's product market. Recall that the owners choose incentive weights simultaneously and that these choices are known by both managers.

To characterize equilibrium contractual incentives, we use the result in Proposition 1 that each firm's expected profits are $E[q_i^2(d_i, d_j)]$ and the results in Theorem 1 describing equilibrium disclosure choices, d_i and d_j. Specifically, condition (C1) in Theorem 1 shows that the effect of delegating production choices on delegated disclosure choices depends on the relation between the ratio $(1 + \lambda_i)/(1 + \lambda_j)$ and 1. If this ratio is larger than 1, then the owners of firm i are choosing incentives to induce their manager to be more aggressive when competing in the product market relative to firm j, and if it is less than 1, then the roles are reversed. Further, the equilibrium disclosure choices are shifted relative to the no-delegation solution, when $\lambda_1 = \lambda_2 = 0$, toward providing the market with more information about the firm whose owners provide fewer incentives to compete aggressively in the product market and away from the firm whose owners provide more incentives.

Theorem 2: For each vector $\delta = (\delta_{ii}, \delta_{ji}, \delta_{jj}, \delta_{ij})$, the ratio of the owners' equilibrium incentive weights is:

$$\frac{1 + \lambda_i^*(\delta)}{1 + \lambda_j^*(\delta)} = \frac{B_i(d_i, d_j)}{B_j(d_j, d_i)},$$

[15] In a previous version of this article, we analyzed the effects of assuming that managers have private information about product demands, the α_i, rather than their own firm's cost structure. When each manager has private information about α_i, each chooses a disclosure strategy that is similar to the strategy described in Theorem 1 with one key difference—the choice is independent of the owners' choices of incentive weights, λ_i, λ_j. Intuitively, the reason is that with the linearity in our model, incentive weights act "as if" the firm's marginal cost of production has been reduced and, thus, only impact the decision to disclose private information about costs.

for $d_i \in \{D, N\}$; $i = 1, 2$; $j \neq i$, and $\delta \in \Delta(d_1, d_2) = \left\{\delta : d_i = d_i^* \text{ and } d_j = d_j^*\right\}$, where d_1^*, d_2^* are defined in Theorem 1 and:

$$B_i(D,D) = E[k_i](2\alpha_i - t\alpha_j - 2E[k_i]) - 2\delta_{ii}^2 Var[k_i] - 2\delta_{ij}^2 Var[k_j]$$

$$B_i(D,N) = [E[k_i](2\alpha_i - t\alpha_j - 2E[k_i]) - 2\delta_{ii}^2 Var[k_i]][E[k_i]E[k_j] + \delta_{ii}\delta_{ji}Var[k_i] + \delta_{ij}\delta_{jj}Var[k_j]]$$

$$B_i(N,D) = [E[k_i](2\alpha_i - t\alpha_j - 2E[k_i]) - 2\delta_{ii}^2 Var[k_i] - 2\delta_{ij}^2 Var[k_j]][E[k_i]E[k_j] + \delta_{ji}\delta_{jj}Var[k_j]]$$

$$B_i(N,N) = 8E[k_i](2\alpha_i - t\alpha_j - 2E[k_i]) - (4 - t^2)\delta_{ii}^2 Var[k_i].$$

Theorem 2 offers expressions for the equilibrium ratio of incentive weights for each of the four possible disclosure regimes because each regime (managers of both firms commit to disclose, the manager of one firm commits and the manager of the other does not, neither manager commits to disclose) is part of a perfect Bayes-Nash Equilibrium for some set of information characteristic vectors, $\delta = (\delta_{ii}, \delta_{ji}, \delta_{jj}, \delta_{ij}) \in \Delta(d_i, d_j)$. To see why, note that our proof strategy is to conjecture a disclosure and associated output choice that are used to compute mutual best-reply incentive weights. If the incentive weights and the original information characteristic vector satisfy the conditions in Theorem 1 and Proposition 1 so that the equilibrium disclosure and output choices are consistent with the original conjecture, then we have a perfect Bayes-Nash Equilibrium to the overall game. To see why all four disclosure regimes occur in equilibrium for specific information characteristic vectors, note that Corollary 1 ensures that disclosure is the unique choice in the private values case. Similarly, Corollary 1 combines with choices of the market intercept and variances to ensure that there are values of the information characteristic vector for which the manager chooses to not disclose.

More importantly, Theorem 2 explains how equilibrium incentive weights are affected by the joint delegation of production and disclosure choices. Perhaps the most important result is that disclosure substitutes for direct contractual incentives. This follows because it is easy to show that $B_i(D, \bullet) < B_i(N, \bullet)$. Thus, for information vectors for which the manager will choose to adopt a policy of disclosure so that $d_i^* = D$, the owners of firm i choose a smaller incentive weight than they choose for information vectors for which the manager will choose a policy of nondisclosure so that $d_i^* = N$, regardless of the incentive weight chosen by the rival.

Theorem 2 also provides a number of useful comparative static results, all of which are derived by examining whether $B_i(d_i, d_j)/B_j(d_j, d_i)$ exceeds 1, equivalently whether $B_i(d_i, d_j) - B_j(d_j, d_i) > 0$, for each information characteristic vector that results in $d_i = d_i^*$. First, if the firms, markets, and information structures are identical except for differences in market size, $\alpha_i \neq \alpha_j$, then the owners of the firm with the larger market offer incentives that induce their manager to compete more aggressively in the product market for all possible subsequent equilibrium disclosure choices. That is, if the firms are otherwise identical, then $\alpha_i > \alpha_j$ implies that $(1 + \lambda_i^*(\delta))/(1 + \lambda_j^*(\delta)) > 1$ in equilibrium for all disclosure regimes, $\delta \in \Delta(d_1, d_2)$. Intuitively, the firm with the larger market gains more from providing its manager with incentives to sell additional units by increasing the incentive weight on revenues relative to its rival. As a result, the information environment is shifted toward disclosure of cost information about the firm with the smaller market share and away from the firm with the larger market share.

Second, for information characteristic vectors that lead to equilibrium disclosure regimes (D, D), (N, D), and (N, N), an increase in one firm's expected marginal costs reduces the incentive

weight the firm's owners put on revenues relative to the weight chosen by the rival firm's owners, keeping prior uncertainty unchanged. Intuitively, if the firm's costs are greater, then the benefits to motivating the manager to produce more than the profit-maximizing quantity are reduced, and so the owners reduce the manager's incentives to produce "extra" output. As a result, the manager is motivated to compete less aggressively in the firm's product market and, all else equal, is more willing to disclose information that is predominantly useful for understanding the firm's marginal costs. Thus, $(1+\lambda_i^*(\delta))/(1+\lambda_j^*(\delta))<1$ when firm i is the high-cost producer, shifting the information environment toward cost information about the high-cost firm and away from cost information about the low-cost firm. Things are slightly different for information vectors that lead to the equilibrium disclosure regime (D, N). In this case, firm i's rival will be fully informed, while firm i will not learn its rival manager's private information. As a result, there is an additional strategic effect that impacts the owners' choice of incentive weight. As long as firm i is not at an extreme cost disadvantage, the results just described remain. Only when firm i's cost disadvantage is extreme do its owners respond by choosing a larger incentive weight on revenues than the rival's owners.[16] This is because as firm i's expected marginal costs increase, its competitive position deteriorates relative to its rival's. This reduces the rival owners' incentive to motivate their manager to produce more, leading them to choose a smaller incentive weight. This, in turn, motivates firm i's owners to offer greater incentive weights to its manager.

Third, Theorem 2 allows us to analyze the impact of differences in *a priori* uncertainty on equilibrium incentive weights and delegated disclosure. Uncertainty is usually measured as the variance of a random variable. However, because the variance depends on both the mean and dispersion and the mean has direct effects, as described above, we will focus on dispersion as measured by a mean preserving spread as our proxy for increased uncertainty. When uncertainty about firm i's marginal costs increases, then $B_i(d_i, d_j)$ declines relative to $B_j(d_j, d_i)$ for $(d_i, d_j) \in \{(D, D), (N, D), (N, N)\}$. Thus, for information characteristic vectors that lead to equilibrium disclosure regimes (D, D), (N, D), and (N, N), the ratio of incentive weights declines and any induced disclosures by firm i's manager are shifted toward disclosing more information about her own costs and less information about her rival's when uncertainty about firm i's marginal costs increases. Intuitively, the increase in uncertainty causes the variance of the manager's output choice to increase, increasing the firm's expected profits and reducing the marginal benefits of inducing additional variation in the manager's output choice through her incentive contract. Because the owners' costs of offering these incentives are unchanged, they reduce the incentive weight they offer the manager.

Things are slightly different for information vectors that lead to the equilibrium disclosure regime (D, N). In this case, because manager i's rival is fully informed while she does not learn her rival manager's private information, an additional strategic effect arises that can alter the owners' choice of equilibrium incentive weights. Specifically, if manager i's private information is predominantly useful in understanding her own firm's marginal costs, so that δ_{ii} is sufficiently bigger than δ_{ji}, then an increase in uncertainty about firm i's marginal costs has the same effects as just described because $B_i(D, N)$ declines relative to $B_j(N, D)$. However, if manager i's private information is predominantly useful for understanding her rival's marginal costs, so that δ_{ji} is sufficiently bigger than δ_{ii}, then the reverse occurs. An increase in uncertainty about firm i's marginal costs results in manager i disclosing more information about firm j's costs and less information about her firm's costs. When manager i discloses her private information and manager j does not, the information advantage provided to firm j is sufficient to cause its owners to increase

[16] The magnitude of the extreme cost disadvantage just mentioned is found by setting $B_i(D, N) - B_j(N, D) = 0$ for information characteristic vectors that lead to the equilibrium disclosure regime (D, N).

the incentive weight on revenues, even though the increased uncertainty about the firm's marginal costs increases the variability of the manager's output choice.

Finally, Theorem 2 offers insight into how the degree of product differentiation affects the joint delegation of production and disclosure choices. In particular, for fixed market sizes, if firm i has smaller expected marginal costs, then the ratio of incentive weights, $(1+\lambda_i^*(\delta))/(1+\lambda_j^*(\delta))$, increases because $B_i(d_i, d_j) - B_j(d_j, d_i)$ increases. As a result, the owners of the firm with smaller expected marginal costs provide incentives that induce their manager to compete more aggressively in the product market and to be more willing to disclose information she has about her rival's marginal costs when the firms sell more differentiated products. Intuitively, the benefits of smaller expected marginal costs are greater when the firms sell less similar products, increasing the marginal benefits of motivating the manager to produce more output and inducing the owners to increase the incentive weight in the manager's compensation contract. Thus, in markets with less differentiated products, there is more information about the high-cost firm and less about the low-cost firm.

Our analysis of joint delegation also offers insights into the impact of the firm's owners anticipating the rival manager's disclosure choice on equilibrium incentive weights:

Theorem 3: If the firms differ only in how useful their private information about costs is to their rival, then the owners choose a larger incentive weight on revenues when the rival does not disclose its private information about costs when compared to the weight chosen when the rival does disclose. For $\delta_1 \in \Delta(\bullet, D)$ and $\delta_2 \in \Delta(\bullet, N)$, $\lambda_i(\delta_1) < \lambda_i(\delta_2)$.

Intuitively, because profits are a convex function of the firm's production choice, the value of the firm is greater when the output chosen by the manager is more sensitive to the available information. The sensitivity to the available information is greater if the rival manager discloses her private information because the firm's manager can tailor her output choice to the rival's private information, as well as her own. However, the sensitivity is also greater if the owners choose a larger incentive weight on revenues. Thus, Theorem 3 tells us that these two sources of sensitivity are substitutes in that the firm's owners opt to cut back on contractual incentives when the *rival's* disclosure will substitute for the "lost" sensitivity.

Before turning to empirical implications of joint delegation, we should summarize the information Theorem 2 provides about the impact of joint delegation on firm disclosures. Specifically, incentives chosen by the firm's owners do not ensure that either disclosure or nondisclosure is always the induced choice by the manager. Rather, equilibrium incentives result in all four disclosure adoption choices (neither firm disclosing, one firm disclosing, the other firm disclosing, or both disclosing) arising for different parameterizations, including different information characteristic vectors. Further, joint delegation alters both the likelihood of disclosure and what will be disclosed. For owners who choose to be the more aggressive competitor in equilibrium, joint delegation increases the likelihood of disclosure and shifts disclosure away from information that is predominantly about the disclosing firm and toward information that is predominantly useful to the rival. Conversely, for owners who choose to be the less aggressive competitor in equilibrium, delegation reduces the likelihood of disclosure. However, if the manager adopts a policy of disclosure, then the incentives provided by the owners lead the manager to shift the information she discloses toward information about her own firm. Thus, delegation shifts the publicly available information so that more is learned about the less aggressive competitor and less is learned about the more aggressive competitor.

Theorem 2 and Corollary 1 also highlight a stark difference between disclosure without delegation and disclosure with joint delegation. Specifically, absent delegation in the private values Cournot competition case, neither firm adopts a policy of disclosing private information, e.g., Gal-

Or (1985, 1986), Darrough (1993), and our Corollary 1. In contrast, with joint delegation, Corollary 1 combines with Theorem 2 to show that owners of one firm may offer their manager sufficient incentives to compete aggressively in the product market that *also* induce their manager to choose to disclose her private information. Thus, with joint delegation, the more aggressive competitor may, in fact, choose a different disclosure policy than would have been chosen absent joint delegation.

VII. EMPIRICAL IMPLICATIONS OF JOINT DELEGATION

In this section, we combine the results from Theorems 1 and 2 to develop empirical implications associated with joint delegation of disclosure and production choices. Focusing first on the implications for contractual incentive weights described in Theorem 2, we have shown that owners of the firm with a larger market or lower costs in expectation place greater weight on the firm's revenues to motivate their manager to be a more aggressive competitor in the product market. Further, for all firms, the incentive weight is smaller when owners anticipate that their manager will provide voluntary disclosure. While our model does not explicitly study a firm's decision to adopt a cost leadership or product differentiation strategy, it does include a measure of the degree of product differentiation t that would be the result of the firms' strategy choices in a richer model. Taking that perspective, our model can be interpreted to predict that firms with larger market shares or those employing cost leadership strategies will offer incentive contracts that depend more heavily on realized revenues than firms with smaller market shares or those employing differentiation strategies. Further, contracts will place smaller weight on revenue performance for those firms whose managers offer more voluntary disclosures. Theorem 2 also shows that these differences arise when comparing young versus mature firms and firms in industries with highly variable production costs versus firms in industries with more stable input prices.

Before turning to the empirical implications of joint delegation on disclosure choices, we emphasize that none of these implications follow without delegation. That is, absent delegation, firm disclosure choices are not affected by expected market sizes, production costs, or *ex ante* cost uncertainty. Specifically, the results in Theorems 1 and 2 imply that firms with larger expected profit margins are more likely to provide voluntary disclosure of production cost information, but less likely to provide voluntary disclosure that is predominantly related to their own costs. In contrast, firms with smaller expected profit margins are less likely to provide voluntary disclosure of production cost information, but more likely to provide voluntary disclosure that is predominantly related to their own costs. As a result, our analysis predicts that more information about the cost structure of firms with smaller expected profit margins and less information about the cost structures of firms with larger expected profit margins will be provided. Because our results on expected profit margins and larger product markets are similar, our analysis also suggests that firms with greater market shares are also more likely to provide voluntary disclosures overall, but less likely to provide voluntary disclosures that are predominantly related to their own costs. Consequently, there will be more information available about the cost structure of firms with small market shares and less information about the cost structures of firms with large market shares. Said differently, our model suggests that a main result of joint delegation on disclosure is that more information about the "weaker" competitor and less information about the "stronger" competitor will be disclosed.

The results in Theorems 1 and 2 also suggest that joint delegation is affected by the degree of product differentiation. Specifically, in markets with less differentiated products, joint delegation implies that disclosure is shifted toward information about the high-cost firm and away from information about the low-cost firm. Further, the degree of production differentiation also moderates the impact of different expected profit margins. The effects of different profit margins

will be smaller in industries with high levels of product differentiation and will be larger in industries with less differentiation. Because the degree to which products are differentiated, the magnitude of t in our model, is often associated with the firms' chosen business strategies, we expect that the magnitude of the effects of joint delegation will be greater for firms with cost leadership strategies and smaller for firms with differentiation strategies.

Finally, Theorem 3 offers new empirical predictions that relate managerial compensation to *rival firms'* disclosures. First, it suggests that managerial compensation will be more sensitive to the firm's revenues in industries where voluntary disclosures are less common and less sensitive in industries where they are more common. Second, it suggests that the difference should be greater in industries with short histories, that is, those with greater prior cost uncertainty. Third, it suggests that the difference should be larger in industries with greater product heterogeneity.

VIII. CONCLUSIONS

We study the joint delegation of production and disclosure choices in a model that synthesizes the delegation of production choices literature, such as Brander and Lewis (1986) and Fershtman and Judd (1987), and the *ex ante* voluntary disclosure literature, such as Gal-Or (1985, 1986) and Darrough (1993). We assume that each manager's compensation depends on the firm's realized revenues and profits, and that both the decision to adopt a disclosure policy and the decision of how much to sell in the product market are delegated to the firm's manager. Our first set of results describes firm characteristics that lead owners to place relatively greater incentive weight on revenues. We also show that these incentive weights differ for managers who choose *ex ante* voluntary disclosure. Specifically, the owners of firms with larger expected markets, lower expected costs, or less uncertainty about production costs choose to place greater incentive weight on revenues, thereby encouraging their manager to compete more aggressively in the product market relative to the rival. Further, the delegation of disclosure, in addition to the delegation of production choices, allows the firm's owners to reduce the incentive weight on revenues for managers who disclose their private information. Intuitively, the firm's owners choose how sensitive managerial compensation is to the manager's production choice. Because this sensitivity is naturally greater if the manager opts to disclose, the firm's owners substitute disclosure for direct incentives. They can also substitute disclosures by their firm's rival for direct incentives. That is, disclosure by *either* manager substitutes for direct contractual incentives.

Our second set of results relates to the impact of joint delegation on the decision to disclose. We find that owners of the firm who provide incentives for their manager to compete more aggressively in the product market also increase the likelihood that their manager opts to disclose and, perhaps more importantly, shift their manager's disclosure away from disclosing information that is predominantly informative about the firm's own production costs and toward information that is predominantly informative about the rival's production costs relative to the no-delegation solution. Thus, joint delegation alters the information environment for competing firms by creating incentives to provide more information about production costs of less aggressive competitors and less information about production costs of more aggressive competitors.

Finally, our analysis of joint delegation also produces a number of testable cross-sectional empirical implications. First, our results suggest that firms in less differentiated product markets, such as firms that adopt cost leadership strategies or that focus on serving mass markets, use incentive contracts that rely more heavily on revenue metrics than do firms that employ differentiation strategies or focus on serving niche markets. Second, joint delegation alters the information environment for the firms in a given industry by reducing the information available about the larger, more aggressive competitors and increasing the information available about the smaller, less aggressive competitors. Third, our analysis suggests that firms whose managers offer

more voluntary disclosures will have compensation contracts that rely less heavily on revenue metrics, and firms whose managers offer fewer voluntary disclosures will have compensation contracts that rely more heavily on revenue metrics. Fourth, our analysis suggests that these same relations hold for firms that have less cost uncertainty, either because key input prices are relatively stable or because the firms compete in mature markets when compared to firms whose key input prices are very uncertain or that are younger or that compete in rapidly changing industries. Finally, our analysis offers an interesting linkage between the disclosure policy of a firm's rivals and the incentive contract chosen by the firm's owners. Specifically, the owners of a firm place greater incentive weight on revenues when *rival firms'* managers are not expected to offer voluntary disclosures. That is, in industries where disclosure by rivals is limited, owners induce their manager to compete more aggressively and increase the likelihood that their manager offers voluntary disclosures.

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APPENDIX A

Proof of Proposition 1

Manager i solves $\max_{q_i} E[(1+\lambda_i)(\alpha_i - q_i - tq_j)q_i - (\delta_{ii}c_i + \delta_{ij}c_j)q_i \mid \phi_i]$, which yields the first-order condition $q_i = E[\frac{1}{2}(\alpha_i - tq_j) - \frac{1}{2(1+\lambda_i)}k_i|\phi_i]$. Each of the cases differ in that the managers' information sets are different. For example, if both have committed to disclose, then both know c_i and c_j and, thus, both know k_i and k_j. In other words, the managers play a game of complete information and we obtain the standard result for $q_i(D, D)$. If manager i has committed to disclose, but manager j has not, then manager i does not learn c_j. As a result, manager i's first-order condition becomes $q_i = \frac{1}{2}(\alpha_i - tE[q_j|c_i]) - \frac{1}{2(1+\lambda_i)}E[k_i|c_i]$ and manager j's is the same as in the full information case. Solving produces $q_i(D, N)$ and, by symmetry, $q_i(N, D)$. In the final case, when both managers commit not to disclose, the first-order conditions are again $q_i = \frac{1}{2}(\alpha_i - tE[q_j|c_i]) - \frac{1}{2(1+\lambda_i)}E[k_i|c_i]$ (where the difference in this case comes from observing that the manager of firm i knows that the manager of firm j does not know c_i, while she does in the (D, N) case). Solving this pair of equations yields $q_i(N, N)$. In each case, substituting the equilibrium quantities into the manager's objective function produces the expression for her payoffs given in Proposition 1. ■

Proof of Theorem 1

Our proof strategy is to show that $E[\pi_i^C(D,N)] > E[\pi_i^C(N,N)]$, and then that $E[\pi_i^C(D,D)] > E[\pi_i^C(N,D)]$, which are the conditions required for disclosure to be the dominant strategy. Similarly, reversing the inequalities yields the conditions required for nondisclosure to be the dominant strategy. Direct computations show that:

$$\begin{aligned}
E[\pi_i^C(D,N)] - E[\pi_i^C(N,N)] &= E[\pi_i^C(D,D)] - E[\pi_i^C(N,D)] \\
&= \left(\frac{-t}{4(1+\lambda_i)^2(1+\lambda_j)^2(4-t^2)^2}\right) \\
&\quad + \Big(-4t(1+\lambda_i)^2\delta_{ji}^2 + 16(1+\lambda_i)(1+\lambda_j)\delta_{ii}\delta_{ji} \\
&\quad + t(1+\lambda_j)^2(-8+t^2)\delta_{ii}^2\Big)Var[c_i].
\end{aligned}$$

Thus, if (C1) holds, then disclosure of private information about production costs is manager 1's dominant strategy. If (C1) fails, then nondisclosure is her dominant strategy. ■

Proof of Theorem 2

Each firm's owners choose the incentive weight to offer their managers simultaneously. Accordingly, we determine their equilibrium choices in the usual way. First, the owners of firm i solve $\max_{\lambda_i(di,\, dj)} E[q_i(d_i, d_j)^2]$, which yields the first-order condition $E[2q_i(d_i, d_j)\frac{\partial q_i(d_i, d_j)}{\partial \lambda_i}] = 0$. These equations are then solved simultaneously for the equilibrium incentive weights. The expressions are most easily simplified by computing the ratio $(1 + \lambda_j)/(1 + \lambda_i)$ to obtain the expressions in the text.

For each vector $\delta \equiv (\delta_{ii}, \delta_{ji}, \delta_{jj}, \delta_{ij})$ selected from $[0, 1]^4$ and each pair (λ_i, λ_j), Theorem 1 provides the Perfect Bayesian Equilibrium payoffs in the game from stage 2 to the end. Substituting the payoffs for the subgames shows that the resulting first stage is a simultaneous play game of complete information, where each firm chooses its incentive weight (λ_i). Standard proofs of existence of a Nash equilibrium ensure that, for each vector δ, there is at least one (λ_i, λ_j) pair that forms a Nash Equilibrium and, when combined with the results of Theorem 1 and Proposition 1, form a Perfect Bayes-Nash Equilibrium to the game we study. Given that an equilibrium exists for each vector δ, we next derive the expressions for the equilibrium λs.

For each δ vector, we first conjecture that the equilibrium choices in the final two stages of the game are that the two firms adopt policies of disclosing their private information (D, D) and output choices $q_i(D, D)$ from Proposition 1. Given this conjecture, owners of firm i solve $\max_{\lambda_i} E[q_i(D,D)^2]$, which yields the first-order condition $E[2q_i(D,D)\frac{\partial q_i(D,D)}{\partial \lambda_i}] = 0$. These equations are then solved simultaneously for the conjectured equilibrium incentive weights. The expressions are most easily simplified by computing the ratio $(1 + \lambda_j)/(1 + \lambda_i)$ to obtain the expression in the text:

$$\frac{1+\lambda_i^1}{1+\lambda_j^1} = \frac{B_i(D,D)}{B_j(D,D)},$$

where:

$$B_i(D,D) = E[k_i](2\alpha_i - t\alpha_j - 2E[k_i]) - 2\delta_{ii}^2 Var[k_i] - 2\delta_{ij}^2 Var[k_j], i = 1,2; j \neq i.$$

Combining the ratio of the λs with the originally selected vector of δs allows us to determine whether condition (C1) of Theorem 1 is satisfied for firm i and whether it is satisfied for firm j. If (C1) is satisfied for each firm separately, then the conjectured disclosure strategies are the chosen disclosure strategies, the conjectured incentive weights are the equilibrium incentive weights, and we have a perfect Bayes-Nash Equilibrium for the chosen δ vector. We collect the δ vectors that form a perfect Bayes-Nash Equilibrium using the (D, D) conjecture into the set $\Delta(D, D)$. That $\Delta(D, D)$ is not empty follows immediately because $\delta = (1, 0, 1, 0) \in \Delta(D, D)$.[17] ∎

Proof of Theorem 3

Tedious calculations using the results on incentive weights from Theorem 2 show that:

$$\lambda_i(\bullet,N) - \lambda_i(\bullet,D) = E[\alpha_i]E[c_i]^5(8-t^2)(4-t^2) + 2t^3E[c_i]^3E[\alpha_i](16-8t-2t^2-t^3)Var[c_i].$$

Because $t < 1$, both the first and second terms are positive. Thus, the incentive weight when the firm's rival manager does not disclose exceeds the incentive weight when the rival manager does disclose. ∎

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[17] The remaining three cases are proved analogously. Proofs are available from the authors upon request.

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Six Decades of Research, Teaching, and Participation in the AAA

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ABSTRACT: These remarks provide some perspective on my six decades of research, teaching, and participation in the AAA. A recurring theme is that my career took several unexpected turns and that my research often had unexpected outcomes. Several areas of research are discussed, including the prediction of financial distress, the information content of earnings announcements, the information content of prices, accounting and market measures of risk, discretion in financial reporting, conservatism, and value relevance of financial statements. Included is a brief summary of some of what I have learned from six decades of teaching. I review some of the major benefits of AAA participation.

Keywords: *financial distress; earnings; prices; risk; discretion; conservatism; value relevance.*

I. INTRODUCTION

When Mary Barth asked me to present some structured remarks around a topic of my choice, I was at a loss. I considered many possible topics, most of which many of our colleagues could do as well as and most likely better than I. So I chose a topic where there were fewer colleagues who could address the topic—at least with first-hand knowledge. I chose "Six Decades of Research, Teaching, and Participation in the AAA."[1]

Whenever possible, I like to choose a theme around which to present some semi-structured thoughts. Having a theme provides at least the appearance of a structure even if there is little or

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[1] In an earlier Presidential Scholar address (Beaver 2002), I addressed five research areas, including several where I was not a direct participant. Here, the focus is considerably narrower in that I focus only on those areas where I have directly participated. I make no an attempt to be comprehensive and, as a result, I have not included many important papers by others. The remarks could be more appropriately titled, "My Six Decades of Research, Teaching, and Participation in the AAA."

none. A few years ago, I participated in the AAA Doctoral Consortium and built my remarks around the Neil Diamond hit, "I'm a Believer." This theme seemed appropriate since my remarks dealt with research in market efficiency. Apparently, the theme I chose was the most remembered part of my talk.

I have decided to build my current remarks around the Rolling Stones' classic, "You Can't Always Get What You Want" and its lyric "but if you try sometimes, you might find you get what you need," which underscores that many stages in my career took unexpected turns.

My remarks also serve a secondary purpose. My research goal has been to combine important conceptual research from related fields with the rich institutional details of the financial reporting environment. The published study is just the tip of the iceberg. These remarks are intended to fill in some of what is below the surface. One aspect of the serendipitous nature of my career is that I was in the right place at the right time. There were research gold nuggets lying on the ground. If I had not picked them up, someone else would have in fairly short order.

In the Beginning

In my family, there was no tradition of attending college. My attending college was a family goal, and the highest level of attainment possible was to attend the University of Notre Dame. I applied only to Notre Dame. To many in my family, the apex of my career was the day I was accepted into Notre Dame. I received an excellent education at Notre Dame. It is one of the three great universities with which I have been fortunate to be associated.

I was a Business Administration undergraduate, and even quite late in my senior year, I had no intention of going on to graduate school. While working as a grader and teaching assistant for Paul Conway, he suggested that I consider an academic career. I had no interest in joining the academic community. There were several professions I admired, including academia, but I had no intention of joining any of them.

Late in my senior year, one of the deans called me into his office. There was a fellowship to The University of Chicago's M.B.A. program that would go to a University of Notre Dame student and the dean wanted me to apply. He was frustrated by that fact that the fellowship each year went to a liberal arts, engineering, or science major, and he wanted someone from the business school to win. I said I had no interest in going to graduate school. However, I now believe that the dean suspected that I would accept the fellowship if I were awarded it.

I filled out the application even though I thought there was little chance of my receiving a fellowship. To my surprise, I received a generous fellowship, which substantially reduced the opportunity cost of being educated. I applied only to The University of Chicago's M.B.A. program. If Chicago did not accept me, I did not have a "Plan B." Applying to only one M.B.A. program is not the hallmark of good decision-making and is inconsistent with the advice I give my children, grandchildren, and my students.

Shortly after graduating from the University of Notre Dame, I joined the M.B.A. program in the summer quarter. It was my intention to finish the M.B.A. program as quickly as possible and enter the business world. During my first year in the M.B.A. program, I also worked three days a week for a mortgage servicing company in the Loop. One of my tasks in mortgage servicing was to conduct face-to-face interviews with borrowers who were 90 days or more delinquent. On a typical day, I would drive 200 miles covering various neighborhoods.

When I was interviewing, I sensed there might be some risk in this occupation. As it turns out, that same day, in another part of Chicago, one of my counterparts working for another mortgage servicing firm was shot. Perhaps there was another profession that was somewhat less risky, and I began to rethink my earlier reservations about an academic career. However, my reasons for reconsidering academe were not primarily negative.

The Doctoral Program

The first year at Chicago was an eye-opener. The faculty was outstanding and the research opportunities appeared unlimited. After my first year in the M.B.A. program, I applied to the doctoral program. The director of the doctoral program jokingly told me he was considering rejecting my application because it was "too sincere." Consistent with my decision-making at the undergraduate and M.B.A. level, I applied to only one doctoral program.

Chicago had an outstanding faculty and doctoral students in accounting, finance, and economics. There was Merton Miller and Gene Fama in finance, George Stigler and Milton Friedman in economics, and, in accounting, George Sorter (my thesis advisor and mentor), Chuck Horngren, Sid Davidson, and Nick Dopuch, among others. Doctoral students in finance included Myron Scholes, Mike Jensen, Richard Roll, and Marshall Blume, and accounting students included Joel Demski, Philip Brown, and, somewhat later, Ray Ball and Ross Watts, among others.

II. SIX DECADES OF RESEARCH

The Doctoral Thesis

The mid-1960s were an exciting time to be a doctoral student. Accounting theory and empirical research, as we know them today, were virtually nonexistent. Major advances were being made in financial economics, particularly with respect to market efficiency and capital asset pricing. Technologically, the advent of computers had enormous effects on sample size and on the sophistication of statistical methods that could be applied to the empirical testing of hypotheses.

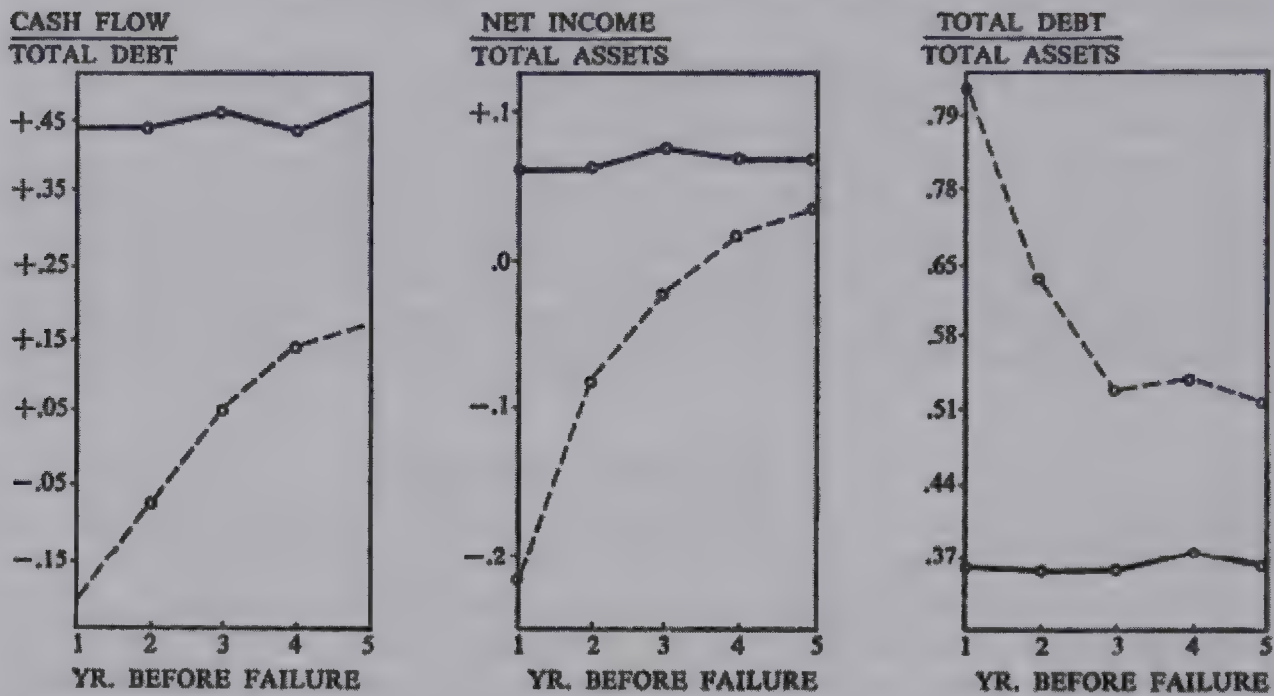
A topic of longstanding interest was the ability of financial ratios to predict bankruptcy and bond default. In courses at Notre Dame, I was intrigued by texts that surveyed the major ratios and offered them as predictors of failure. I also spent one summer working for Dun & Bradstreet, reviewing hundreds of reports. I spent another summer in the business school library tracing the history of ratio analysis.

Prior to the advent of computers, it would have been virtually impossible to conduct an empirical study on the scale of my thesis. Having said this, the Compustat tape was in an embryonic state and virtually none of the failed firms was on Compustat. I had to hand-collect all of the data for the thesis. It was the "right place and time" because accounting research was in transition, moving from a traditional approach toward the scientific method and empirical tests of longstanding beliefs about the role of financial statements.

In the early stages, I sought out reactions of others to the idea. One finance faculty member told me that it was unnecessary to conduct an empirical study because we already knew what the results would be. I said, "Really? What would that be?" Well, I was told that I would find that financial ratios would have no ability to predict bankruptcy because of all of the limitations of accounting data. I could not expect such data to predict an important economic event such as bankruptcy. I was advised to explore a topic where we did not know the results in advance.

A few days later, I met with one of the lending officers of a major Chicago commercial bank. He told me that there was no point in conducting an empirical study because we already knew what the results would be. I said, "Really? What would that be?" I was going to find that financial ratios were excellent predictors of bankruptcy, and that is why they were in such widespread use. At this stage, I knew my results would be a big surprise to somebody, regardless of what the results were. This anecdote underscores the fact that the lack of empirical research at that time permitted a wide dispersion in the beliefs about an important topic—the predictive ability of financial statement information with respect to financial distress.

FIGURE 1
Profile Analysis: Comparison of Mean Values



Source: Beaver (1966, 82, Figure 1).

The study produced some great pictures. One such picture is the difference in the means between failed and nonfailed firms. I initially conducted an analysis of a difference in means, as reported in Figure 1 (Beaver 1966). A plot of mean differences was a convenient descriptive tool. Even five years before failure, profitability was lower, leverage was higher, and cash flow to total liabilities was lower.[2] Moreover, as the year of failure approached, the difference in means widened.

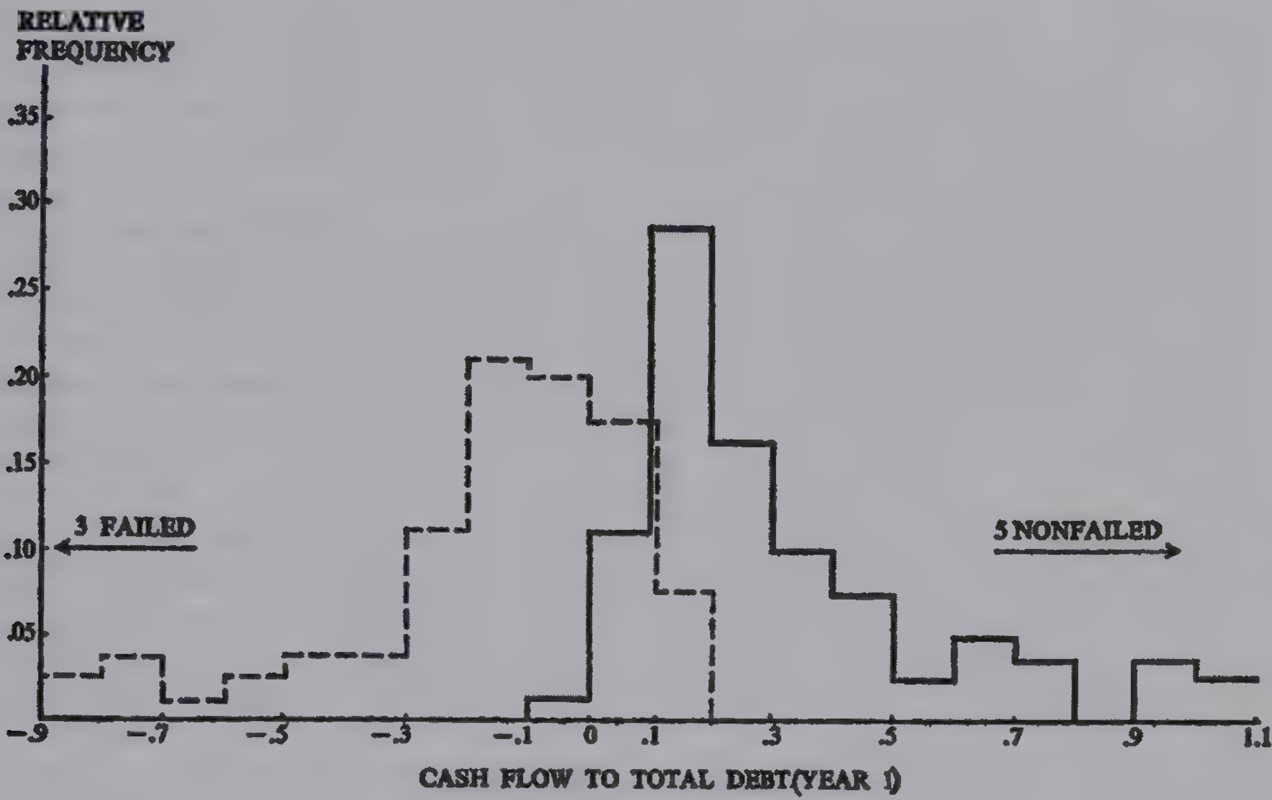
The difference in means was encouraging for finding predictive ability. In the absence of a difference in means, it is difficult to imagine that there would be predictive ability, but the difference is hardly determinative. Among other things, *ex post* knowledge of the failure status is used to compute the means. Also, the dispersion around the means would be critical. Figure 2 (Beaver 1966) reports the histograms of the failed and nonfailed firms. There is very little overlap between the two histograms. One of the innovative features of the study was adopting a Bayesian approach. The likelihood ratios implied by the heights of two histograms in each interval are dramatic. Taken at face value, the likelihood ratio in favor of failure ranges from zero to infinity.[3]

I also conducted a multivariate analysis, but could not identify a multivariate model that outperformed the best single ratio on an out-of-sample test. This finding is not what I wanted, but I

[2] The "cash flow" variable was operationally defined as Earnings before Interest, Taxes, Depreciation, and Amortization (EBITDA).

[3] However, this extreme range is essentially an artifact of the small paired-sample design. In the population, there are approximately 100 nonfailed firms for every failed firm, which means there would be a small, but positive, probability density in the lowest values of the nonfailed distribution of firms. Later research (Beaver, McNichols, and Rhie 2005; Beaver, Correia, and McNichols 2010, 2012b) on larger samples confirms that the range of implied likelihood ratios is large, but not infinite.

FIGURE 2
Histogram



Source: Beaver (1966, 92, Figure 2).

did get what I needed. I presented these results at the *Journal of Accounting Research*'s (*JAR*) Empirical Research in Accounting Conference. This conference provided a perfect platform for a young researcher to present findings. Many subsequent researchers have benefited from this platform as well.

The Information Content of Earnings

While the prediction of failure study empirically documented a widely held belief, interest in failure studies had a somewhat narrow audience. The role of accounting information in security price formation offered a broader audience. Exciting concurrent developments in financial economics were of great assistance in constructing a research design that would relate accounting information and security prices. Financial economics permitted a dramatically different way of looking at accounting data.

To appreciate this development, it is important to understand what "accounting theory" was at that time. Accounting theory consisted of measurement models of assets and earnings based on some variation of fair values. Asset and income measurement under market value accounting is straightforward under perfect and complete markets (Beaver and Demski 1979). Although that term had not yet come into existence, the various forms of income theories implicitly assumed that markets were imperfect and/or incomplete. Income theories offered a variety of fair value alternatives, such as entry value, exit value, and net realizable value, among others. There was general agreement that historical cost was deficient, and examples abounded of where a particular

form of fair value was better than historical cost and better than the alternatives suggested by other income theories.

While these excursions were insightful and important to our thinking about accounting measurement, the prospect of trying to become yet another theorist proposing yet another dial turn on fair value was not appealing. It was more appealing to become an empiricist. Moreover, taking an alternative economic view of accounting data as a form of information provided a dramatically different way of looking at the world. This perspective led to "The Information Content of Annual Earnings Announcements" (Beaver 1968).

Ball and Brown (1968) were concurrently conducting their path-breaking research using forecast models of annual earnings. By contrast, I wanted a measure that would reflect the market's reaction to the information immediately prior to the earnings announcement. A database of earnings expectations did not exist at this time; I/B/E/S and analogous databases were not available until many years later. Even when the forecasts are available, they have limitations for this purpose (Beaver, Cornell, Landsman, and Stubben 2008). I wanted a security price-based measure of the stock price reaction that did not depend on specifying a measure of unexpected earnings just prior to the announcement and, therefore, did not attempt to predict the direction of the price movement. I wanted to predict only that changes in equilibrium prices were larger or more likely to occur at earnings announcement times than at other times. Moreover, I felt it was important to develop a measure of information content in addition to a security price-based measure. I wanted a measure that would reflect whether the change in beliefs was sufficient to induce a change in actions. This list of what I wanted was a tall order. It remained to be seen if it was what I needed.

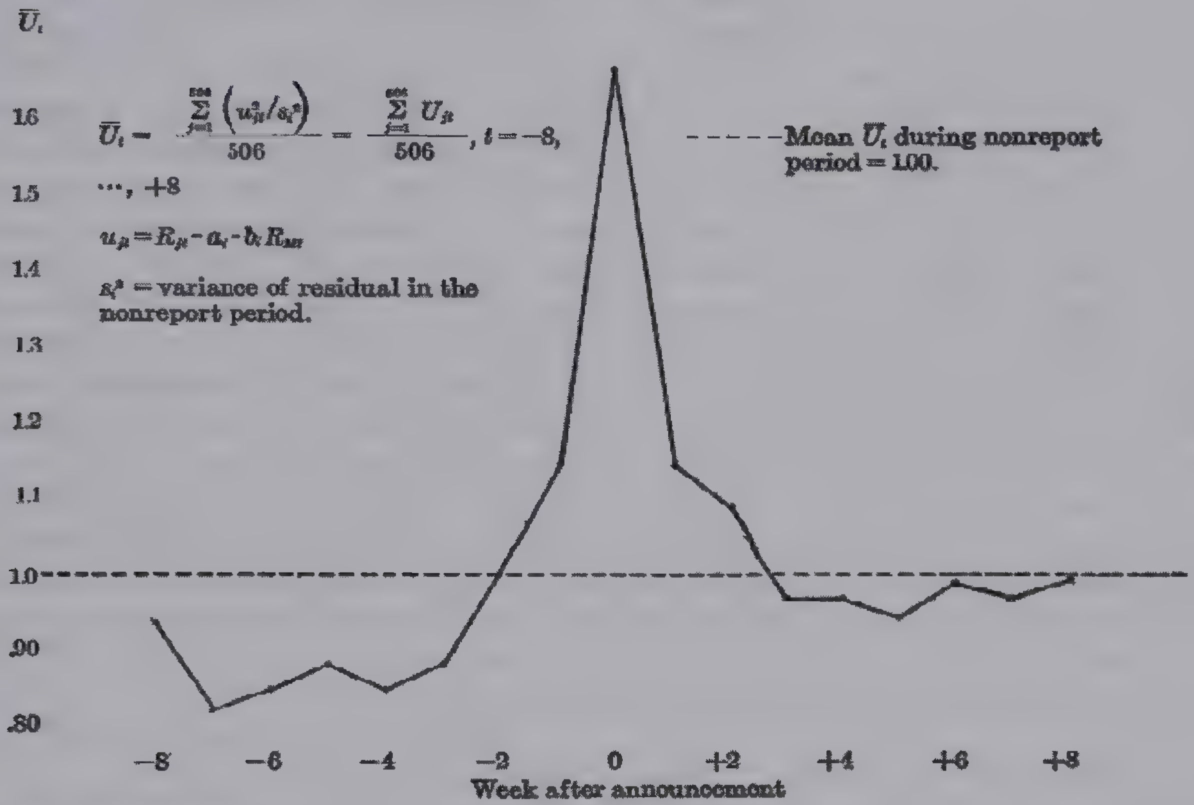
I relied upon intuitive notions of information content. This was necessary because, at the time, the information economics research, as we know it today, was in an embryonic state. I relied on two simple notions: (1) a necessary condition for a signal to have information content is to change beliefs, and (2) moreover, the change in beliefs must be sufficient to induce a change in actions taken (at least for some signal outcomes).

If earnings announcements systematically change investors' beliefs (e.g., viewed as favorable by most investors), then it is intuitive to expect a change in price. If we had a precise measure of the market's expectations of earnings immediately prior to the announcement, then it could be possible to predict the sign of the price change. However, given that such a variable is not observable, I wanted an unsigned price change measure and chose the residual price change squared. Because the measure was pooled over time and across firms, I wanted the measure to be scaled. The squared residual price change at the earnings announcement was divided by the average squared price change at other times during the year (i.e., at nonreport times). Under the null hypothesis and appropriate distributional assumptions, the mean ratio (the U-statistic) would be approximately 1 (Patell 1976). However, my tests were nonparametric and involved testing whether the mean U-statistic in the report period is significantly different from the means in the nonreport period. The U-statistic is a measure of relative price variability. It compares the information content arriving during the earnings announcement time with other "nonreport" times of the year. It is inherently a *relative* measure.[4]

Changes in beliefs need not be systematic across investors. In a world with heterogeneous beliefs, unexpected earnings announcements may be good news for some investors, but bad news for others. In this case, everyone's beliefs may have changed, but there is no change in price. I wanted a second measure that would reflect this situation. Also, I wanted a measure that would

[4] The "nonreport" period is not a "no information" period, but rather reflects information arriving during times other than at the earnings announcement period.

FIGURE 3
Price Residual Analysis



Source: Beaver (1968, 91, Figure 6).

reflect whether the change in beliefs was sufficient to induce a change in action. For example, in a world of homogeneous beliefs, the arrival of earnings information could dramatically affect beliefs, but there would be little or no change in portfolios held because there was universal perception that prices already reflected the implications of the information. For my operational measure of change in actions, I choose a volume (shares traded) measure.

Of course, it is quite possible to observe four possible scenarios: (1) no price reaction, no volume reaction, (2) a price reaction, but no volume reaction, (3) no price reaction, but a volume reaction, or (4) both a price and a volume reaction. As I discuss in Beaver (1968), each of these scenarios would be telling us something different about the world. I felt I had my bases covered because I had an interpretation no matter what the results were.

It would have been a lot easier if I had chosen to use monthly data as prior research had, because of the availability of the monthly CRSP tape. However, I decided that monthly data would be too coarse. I chose weekly data, but no weekly return or volume file existed. Such information was buried in a mysterious file called the CRSP Master File. Reading the file was beyond my programming skills, so I hired an undergraduate physics major who claimed to be one of the world's best programmers. It took several weeks of false starts to finally produce a file of weekly returns and volume for my sample firms.

At this stage, I was a week away from the deadline for submitting my paper to the *JAR* Conference, and had no results. The research assistant (RA) dropped out of school, so I not only had no results, but I also had no RA. This is not what I wanted, but I learned enough FORTRAN to produce the initial results. Results are reported in Figure 3 (Beaver 1968) for the price change variable (U-statistic). Similar results were reported for the volume measure.

For both the volume and U-statistics, the mean in week zero, the earnings announcement week, was above the mean in the nonreport period and statistically significant using a nonparametric test. In particular, the week zero mean U-statistic was 1.67. The mean was approximately 67 percent higher than the mean in a nonreport week.

There was both a price and volume reaction, and both were significant based on a comparison of the distribution of these statistics in the nonreport period. The use of this form of nonparametric testing approach was important because the distributional properties of a volume measure were largely unknown and unlikely to obey normality. Also, at the time, the normality assumption for price changes was being challenged. Today, data-intensive nonparametric approaches have become common, and the choice of a nonparametric approach was a fortuitous research design decision.

In retrospect, weekly data was also too coarse a partition. While 1.67 was the U-statistic mean value for week zero, it is itself an average of the trading days in the week. If the effect is concentrated largely on one day (day zero), the daily U-statistic is potentially several times higher. Morse (1981) provides some evidence on this issue. As I discuss later, one of my current research projects is investigating further the one-day reaction to earnings announcements.

In this study, I did not get what I wanted, because I did not want to lose an RA one week before the paper deadline. However, I did get what I needed. I was able to turn in a paper with results and present it at the upcoming *JAR* conference. As a further bonus, subsequent research has shown that the mean U-statistic is robust over time and is, in fact, increasing. The U-statistic (or some transformation) and the volume metric (or some transformation) are two of the three major measures of information content used in subsequent research.

The Information Content of Security Prices (Part 1)

During the 1970s, I continued to explore various aspects of the relation between security prices and accounting data. However, in 1980, I began what has turned out to be a tetralogy of research studies under the umbrella of "The Information Content of Security Prices," which we thought would be a clever play on words of the earlier article.

In Beaver, Lambert, and Morse (1980b), the central idea was to invert the familiar earnings-price relation and ask what security prices can tell us about future expected earnings. The paper adopts a rational expectations perspective, which has had dramatic implications for financial economics and macroeconomics.

Under a rational expectations perspective, the price today is a function of expectations of future realizations of value-relevant events, such that, on average, the realizations will be equal to the expectation. In particular, prices today can be characterized as a function of future expected earnings. The ultimate goal of the study was to build a research design where the implicit forecasts of future earnings could be extracted from prices, although we took a convoluted way of getting there.

This perspective permitted us to incorporate a central feature of the financial reporting environment, delayed recognition. The accounting system delays recognition of many events that are publicly known and reflected in prices. Examples are product innovations, contract awards, mineral discoveries, changes in the fair values of many assets and liabilities, and excess present-value projects, among others. Our ultimate goal was potentially to improve earnings forecasts by using a broader information system (relative to past earnings). Major earnings forecasting models developed in the 1970s (e.g., Ball and Watts 1972) were conditioned on realizations of past earnings. We wanted a simple variable that would parsimoniously capture a broader set of conditioning variables.

Security price was chosen as a parsimonious proxy for other publicly known information with implications for future earnings, but not necessarily extractable from the past earnings series. A

simple stochastic process was assumed for ungarbled and garbled earnings, and an expression of the price-earnings relation was derived that contained unwarranted garbling terms.[5] An expression was derived relating percentage changes in price to percentage changes in earnings (Beaver et al. 1980b, 10, Equation 7a).

The challenge was to remove the garbling terms from the expression, which appeared in both the numerator and denominator of the expression. While there are a variety of possible approaches available, we chose to remove the garbling in earnings by an aggregation of the data by the dependent variable, security return.[6] Aggregation of the data is a commonly used approach to remove measurement error from the data. The objective is to find a variable that is highly correlated with ungarbled earnings and uncorrelated with the garbling component. Security returns seemed like the ideal candidate. We expected that the ERC on the nonaggregated data would start low (based on prior research) and would increase as aggregation increased.[7] To my surprise, the ERC passed 1 and kept climbing and approached 2. This is not what we wanted. The question is—is it what we needed?

We invoked an innovative temporal aggregation story that would explain this result, albeit after the fact. Needless to say, it is typically dangerous to provide theories for unexpected results. Moreover, we were mystified that, while aggregation by explanatory variables was a rather common solution to the measurement error problem, we could not find any source that aggregated by the dependent variable. We could only wonder—why? But we got what we needed—at least for now.

Information Content of Prices (Part 2)

Beaver, Lambert, and Ryan (1987) take a second look at the information content of security prices. We had a lingering suspicion about aggregating on the dependent variable. Our analysis indicated that aggregating by the dependent variable would lead to aggregating by the residual term in the ERC regression and would lead to a biased estimate of ERC. Needless to say, this was disconcerting and could explain the increase in the coefficient for higher levels of aggregation. As we pursued the analytics further, the coefficient on the aggregated data turned out to be something familiar. As one aggregated, the coefficient became asymptotically equivalent to conducting a reverse regression (and taking the reciprocal).[8]

The empirical results were consistent with econometric theory. The RRC was approximately 0.50, the reciprocal of which is 2.0, the ERC estimate obtained on aggregated data in the earlier study. Moreover, the coefficient of 0.50 is also consistent with a temporal aggregation interpretation.

The reverse-regression analysis has several benefits. First, it means that the same result could have been obtained with individual security-level regressions without aggregating and without

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[5] A garbling term was added to the analysis because of a potential mismatch between observed earnings response coefficients (ERCs) and theoretical ones. Further, practically speaking, an earnings may contain some value-irrelevant earnings components.

[6] In this formulation, security returns (or price changes) are the dependent variable and percentage changes in earnings are the explanatory variable, which is consistent with the treatment of prior research. The slope coefficient from this regression is known as the earnings response coefficient (ERC). This formulation will be referred to as the ERC regression.

[7] The earnings response coefficient is the slope coefficient from a regression of percentage in price adjusted for dividends (return) on percentage change in earnings. Time-series studies of annual earnings (Ball and Watts 1972) suggest that annual earnings is well approximated by a random walk (with a drift) process.

[8] In a reverse regression, percentage change in earnings is the dependent variable and security returns is the independent variable. The slope coefficient in the reverse regression is referred to as the return response coefficient (RRC).

choosing the level of aggregation. Second, a reverse regression is in the spirit of the information content of prices. If we had thought through the implications of the information content of prices perspective at the outset, we would have written the first paper with a reverse-regression approach. Third, the reverse regression places the garbling of earnings into the residual term and does not induce bias in the coefficients. The ERC regression places the garbling in the explanatory variable and is more problematic. Fourth, with earnings as the dependent variable, it is straightforward to also include lagged price changes. In fact, lagged values of percentage changes in price were found to be statistically significant, which is consistent with the information content of prices perspective and with the contention that price changes reflect information earlier than earnings do. Beaver et al. (1987, Table 4) reports significant lagged price change coefficients.

The second look was not what we wanted, but we got what we needed. We could have avoided a second look by having pursued the matter further in the first article. However, it was better that we wrote the follow-up article than someone else, because someone else may not have been as understanding to the authors of the initial article.

The Information Content of Prices (Parts 3 and 4)

Beaver, McNally, and Stinson (1997) took a third look at the issue in "The Information Content of Earnings and Prices: A Simultaneous Equations Approach." While the reverse-regression approach had been adopted by several studies, the predominant regression method continued to be the ERC approach. Considering the pair of equations (ERC and reverse) led us to a broader perspective. We believed that neither single equation can be the correct formulation, if prices and earnings are being jointly determined, as a literal interpretation of the pair of equations suggests.

Consider why price changes and earnings changes may act as if they are being jointly determined. While both are in part being influenced by factors that do not affect the other, both are in part jointly determined by a set of informational variables that are difficult to specify explicitly. Hence, from this perspective, price changes and earnings changes act as if they are endogenously determined. Either single-equation approach is subject to issues of identification and simultaneous equations bias. Using a system of simultaneous equations, we found that the earnings (ERC) and return (RRC) sensitivity coefficients were higher than under a single-equations approach. Further, the product of the ERC and RRC was closer to 1, as predicted under some theoretically appealing assumptions.

A simultaneous-equations approach has several potential advantages: (1) it addresses issues of identification and bias, (2) it places in proper perspective the prior observations that the R^2s from the regressions are "low," and (3) the estimated earnings and return response coefficients are closer to their theoretically predicted values. The models used were "toy" models in the sense that they were designed to illustrate the approach.

Beaver, Landsman, and Owens (2012a) returned to the issue of simultaneous equations. In the interim, individual studies had continued to use single-equation approaches; either an ERC model or an RRC model, but not both. Here, we explored the joint use of models that appeared in prior research. The RRC equation chosen was the asymmetrical timeliness model of Basu (1997), and the ERC equation chosen was the asymmetrical persistence model of Hayn (1995).

Joint estimation turned out to be a major econometric challenge (i.e., "nightmare") because of the piecewise linear and, hence, discontinuous nature of the equations. Among other issues, we had to carefully avoid the "forbidden regression" misspecification. After much effort by Ed Owens, we finally "solved" the problem. At least, we hope we did. As it turns out, the basic findings of asymmetrical timeliness and asymmetrical persistence are observed in the simultaneous-equations framework as well. However, in the case of asymmetrical timeliness, the response coefficients were

higher for both positive and negative returns. Furthermore, whether one concludes that asymmetrical timeliness has increased or decreased over time depends on whether a single equation or joint set of equations approach is adopted. We believe that a system of equations approach is conceptually better specified and has the potential to make a difference in some contexts. However, this study closes a journey of more than 30 years (1980–2012) on the issue of the information content of prices—at least for now.

Accounting and Market Measures of Risk

Beaver, Kettler, and Scholes (1970) explored the association between accounting-based and market value-based measures of risk. In particular, the article examines systematic risk, as measured by beta. At that time, finance research provided two major findings: (1) market efficiency was a good approximation of the empirical behavior of security prices. In particular, early research challenged conventional wisdom regarding how quickly and with what degree of sophistication stock prices responded to accounting events; and (2) the Capital Asset Pricing Model (CAPM) appeared to be a good approximation of the relative pricing of securities. In particular, under the CAPM, the only risk that was relevant at the individual security level was systematic risk, beta, because unsystematic risk could be diversified away. Under the CAPM framework, beta was conceptually defined and empirically measured.

This early research implied a reduced emphasis on using accounting data to identify overpriced and underpriced securities. However, the research provided a new and potentially important role for accounting data—the prediction of systematic risk.

In examining the association between accounting risk measures and beta, it was important to distinguish between two roles—the macro role and the micro role. The macro role addresses the role of accounting data in the formation of equilibrium prices. The micro role examines the role of accounting data for an individual decision maker (e.g., investor) in predicting the systematic risk of various portfolios and securities in an opportunity set.

An individual investor has the opportunity to use market-based measures, such as historical betas, as well as accounting data and other information. However, the market as a whole does not have the same opportunity because accounting data may be part of the total information mix reflected in market prices. In other words, to the extent that accounting data play a role in the determination of market prices, they are inherently endogenous with respect to accounting data. We found that accounting data played both macro and micro roles. In retrospect, the major role of accounting data may be in the setting of equilibrium prices—the macro role.

The importance of distinguishing between macro and micro uses is important in other contexts as well. For example, consider the use of accounting-based and market price-based models of failure prediction. Evidence indicates that both types of models predict failure well. Moreover, well over 90 percent of the explanatory power of market-based financial distress models can be captured by a relatively parsimonious accounting-based model (Beaver et al. 2005, 2010, 2012b). Of course, for nontraded firms, market-based models are not feasible, and there is an obvious micro use for accounting models. However, even in the case of publicly traded firms, the market price-based default prediction models (e.g., Merton-Black-Scholes-type models) use endogenous variables that are determined by the information available, including accounting data. Hence, accounting data play a macro role here as well.

Conducting this empirical study was a challenging exercise for several reasons: (1) many of the traditional accounting risk measures predated beta and arose in a prediction of failure context. It was a leap to extend their use to beta; (2) beta was clearly a variability measure, but the traditional accounting risk variables were not. We attempted to partially address this by constructing

accounting-based variability measures; and (3) in a related vein, betas could be calculated on a daily basis, while accounting measures were available only quarterly at best.

The analysis was quite primitive and the conceptual link was intuitive rather than formal. However, the study found a significant relation between beta and accounting risk measures, including dividend yield, leverage, and earnings variability. The distinction between macro and micro roles was one of the more important contributions of the study.

Supplemental Disclosures and Discretion in Accruals

My research has also investigated various aspects of discretion in accrual accounting, including the role of supplemental disclosures. Accrual accounting is at the heart of our financial reporting system, particularly with respect to nonmonetary assets and many liabilities (such as pension obligations). There are at least two central features of accrual accounting. First, because of the constraints of Generally Accepted Accounting Principles (GAAP), accounting items often do not reflect all of the available economically relevant information, much of which is publicly available. Second, because judgment is inherently involved in estimating the accrual, discretion is inevitable. Judgment is inherently involved because many accruals reflect implicit predictions about the future. The exercise of that discretion can either enhance or obscure the information contained in the accounting item, depending on the motivations of those exercising discretion.

Supplemental disclosures can play a key role in providing evidence with respect to the discretionary nature of accrual accounting and its effects. Supplemental disclosures are of interest in and of themselves, because evidence of their role in valuation can also provide evidence on the richness of the information used by investors and the degree of sophistication investors use in processing supplemental information, including footnotes.

Much of my research in discretionary accruals has focused on either the banking sector or the property and casualty insurance sector. I focused on these sector-specific studies because there are limits to what one can learn from generic accrual studies, where the accruals may be arising from a variety of transactions. Sector-specific studies "put a face" on the nature of financial reporting and the accrual process.

The banking and property-casualty sectors offer special research opportunities: (1) each sector is relatively homogeneous, at least in terms of asset and liability structure; (2) there is one accrual account of a specialized nature that tends to dominate—allowance for loan losses in the case of banks, and policy loss reserves in the case of property-casualty insurers; (3) both industries are regulated, which adds to the homogeneity of their disclosures; and (4) both sectors provide supplemental disclosure about the large, dominant accrual account.

Banking Sector Research

The first excursion into discretionary accruals was inadvertent (Beaver, Eger, Ryan, and Wolfson 1989). The original idea was to explore the role of nonperforming loans, a supplemental disclosure, and the issue of discretionary behavior was subordinate at best. This study is a classic example of "You Can't Always Get What You Want. . .but if you try sometimes, you might find you get what you need."

The central idea in positing a role for nonperforming loans was that GAAP for loan losses was a non-present value, loss of principal concept. Economic loss, which is the present value of lost principal and interest payments, could differ considerably from the recorded loan losses, even absent discretion. Moreover, nonperforming loans (NPL) could potentially provide information on loss of economic value not reflected in the recorded book value loan amount.

Our major prediction was that the coefficient on nonperforming loans would be negative and significant. Almost as an afterthought, we decided to include Allowance for Loan Losses (ALL) as

a control variable. ALL could potentially reflect a portion of the economic loss to the extent that it was correlated with the unrecorded economic loss. If so, the coefficient on NPL could be diminished and, in the limit, equal 0. The inclusion of both variables would permit us to examine the incremental contribution of each. The NPL coefficient was negative and significant as expected, but unexpectedly, the ALL coefficient was significantly positive. One interpretation was that the additional reserving was viewed by the market as a positive signal about the future earning power of the bank. Wahlen (1994) provides evidence consistent with this interpretation.

Using a two-stage approach, further research in banking shows that the supplemental disclosures can be used to partition loan loss allowance into discretionary and nondiscretionary components. The market appears to price the nondiscretionary portion negatively and the discretionary portion less negatively (Beaver and Engel 1996; Beaver and Venkatachalam 2003).

While there are several advantages to studying the banking sector, there are some disadvantages. A major disadvantage is that the revision to the allowance for loan losses may occur for a variety of reasons. However, there is a sector that provides a more detailed breakdown of the accrual account that permits the researcher to decompose the revision.

Discretion in the Property and Casualty Sector

A major accrual in the property and casualty sector is the policy loss reserve, which reflects the losses expected because of claims arising from policies written in the current period and prior periods. In any given period, the changes in the policy loss reserve reflect two major components: losses expected from policies written in the current period and revisions of losses arising from policies written in prior periods. The revisions can occur for two reasons: (1) the estimates in earlier years were *ex ante* unbiased, but unexpected subsequent events require revision; (2) the estimates in the earlier years were *ex ante* biased, and subsequent revisions are reversing that earlier discretion. Disclosures in this sector are unique and particularly helpful for two reasons: (1) the change in a given year is decomposed into current-period claims and revisions of prior years' estimates; (2) the revision of prior years' estimates is allocated back to each of the respective prior policy years that led to the revision. The revisions of prior years are euphemistically called "development."[9]

If the estimate in a given year is unbiased, the expected value of subsequent development is 0. Moreover, the serial correlation of development is expected to be 0. Beaver and McNichols (1998) found that there is positive first-order serial correlation in revisions, consistent with companies taking more than one year to reflect the revised estimate. Moreover, we find that the market is aware of this serial correlation and embeds it in its valuation of property and casualty companies (Beaver and McNichols 2001). In addition, the market not only prices development, but also appears to fully reflect it. Further, Beaver, McNichols, and Nelson (2003) examine the relation between development (DEV) and earnings, as shown in Figure 4. Figure 4 shows that firm-years with small profits have subsequent upward revisions in the policy loss reserve (i.e., positive DEV), consistent with profits being overstated in the earlier years, perhaps due to loss avoidance. Moreover, firm-years with large profits have subsequent downward revisions in policy loss reserves (i.e., negative DEV), consistent with profits having been originally understated, as predicted by income smoothing.

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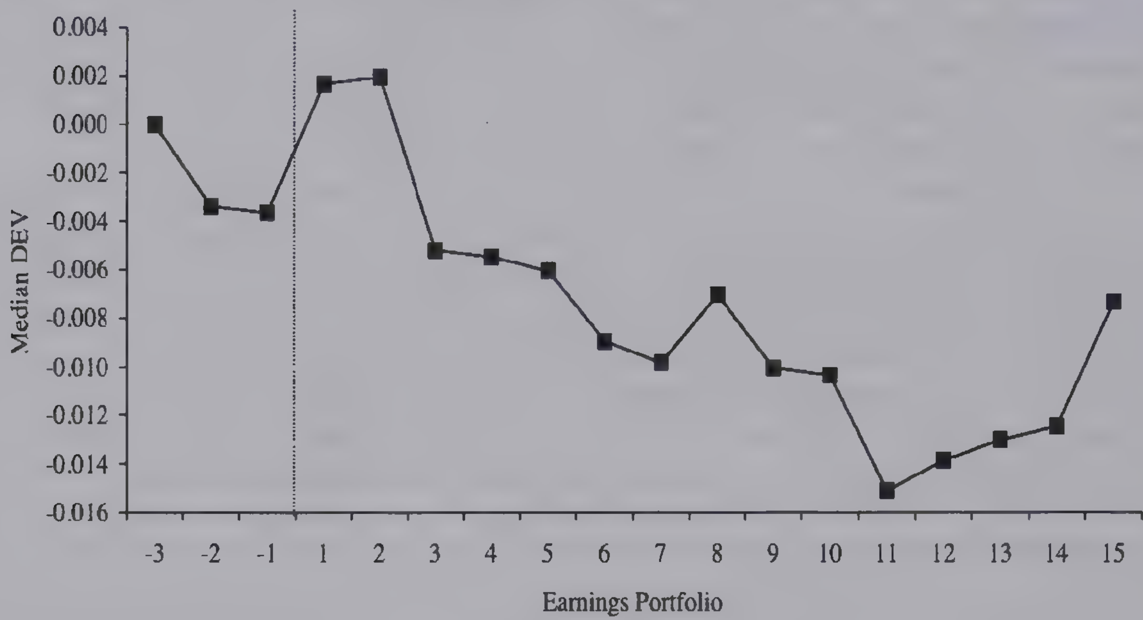
[9] A critical research issue in discretionary behavior studies (e.g., income smoothing) is to compare the income as reported with an estimate of what income would have been reported in the absence of discretion. In many contexts, the derivation of the "as if" income is a challenging, if not impossible, task. The development data permit a unique opportunity to derive an "as if" number.

FIGURE 4
Median Percentage Loss Reserve Development



Source: Beaver et al. (2003, 357, Figure 2).

Conservatism in Accounting

A pervasive feature of financial reporting is conservatism. Some conservatism is unconditional, induced by the nature of GAAP. Two examples are the use of historical cost accounting rather than present value of future cash flows, and the immediate write-off of many classes of expenditures on intangible assets. Other sources of conservatism are conditional, induced by principles, such as "anticipate losses, but no gains" and "lower of cost or market" rules.

It is important to understand what information is consciously omitted from financial statements because of the nature of GAAP. Among other things, it helps to explain market prices leading accounting data (e.g., the information content of prices), the time-series behavior of market-to-book and price-to-earnings ratios, the time-series behavior of earnings, and differential earnings response coefficients to profits versus losses.[10]

Beaver and Ryan (1993) explore the implications of conservative accounting for the behavior of market-to-book ratios (or, more precisely, book-to-market ratios). The book-to-market ratio was viewed as a function of lagged market value changes (i.e., returns). The essential idea is that market price changes lead accounting numbers. However, the introduction of the notion of conservatism permits us to make predictions beyond the generic observation that prices lead earnings. In particular, the structure of lagged price changes would be expected to vary with the magnitude of depreciable assets and average estimated useful life.

Beaver and Ryan (2000) predicted that understanding differing types of conservatism would not only affect the ability to predict book-to-market ratios, but also the ability to predict accounting return on equity. Beaver and Ryan (2005) explicitly decomposed conservatism into conditional and unconditional conservatism—a distinction implicit in our earlier research. The

[10] Beaver (1970) examines the times-series properties of scaled accounting earnings as a function of lagged price changes.

decomposition led to differential predictions regarding book-to-market and future accounting return on equity. In particular, unconditional conservatism would lead to less reversion over time in book-to-market ratios and accounting return on equity compared to conditional conservatism.

Beaver and Ryan (2000) had a long gestation period. In 1996, assuming our paper would be published before 2000, I was worried that I would not have any publication in the 21st century (or the next decade). It is another example of "You Can't Always Get What You Want. . . but if you try sometimes, you might find you get what you need."

Value Relevance Research

Value relevance research with Mary Barth and Wayne Landsman has been a major research area over the last 20 years. Value relevance research examines the association between a security price-based dependent variable and a set of accounting numbers. An accounting number is said to be value-relevant if it has a significant relation to the dependent variable in the predicted manner.

One characteristic of the value relevance research is the incorporation of the rich institutional features of financial statements. It is intended to give empirical content to underlying measurement concepts such as relevance and reliability. The use of a measurement perspective of accounting standards is more likely to be incorporated into the motivation for the research. In doing so, this research attempts to blend traditional accounting theory, as reflected in many accounting standards, with empirical research methods.

One feature is that the timing of the information is not the dominant issue. This feature is a contrast to the information content studies that focus on security returns over relatively short periods of time surrounding the announcement of the information, of which event studies are classic examples. As a result, many value relevance studies have the level of security price, rather than changes in security price, as the dependent variable. This contrast, in part, explains the controversial nature of the work. The informational approach has become deeply ingrained in our research tradition. When event study-style research was first introduced, many did not view it as accounting research (Ball and Brown 2014).

Why is timing not a dominating issue, as it is in information content studies? The market for information may be such that participants seek prior information about the forthcoming accounting number (e.g., earnings). In the limit, an accounting number may be completely preempted by efforts to gather information about the accounting number. Moreover, accounting numbers may not be the unique representation of the underlying construct. In the limit, a vector of competing variables may be highly correlated with the accounting number. However, a key role of financial statements is to summarize value-relevant events in a parsimonious manner, even if the financial statements are not a unique representation.

Value relevance research assumes that it is important to know how well accounting numbers perform this role, even in the presence of competing variables for the same underlying construct. This is in contrast to the informational approach, which requires that the accounting signal not be preempted by more timely information and competing contemporaneous variables. The choice of perspective has important research design implications, such as the choice of levels versus first differences in the market price-based variable. The choice of perspective also forces the researcher to consider whether additional explanatory variables are competing variables or merely proxies for omitted variables.

Value relevance research provides evidence as to whether the accounting numbers relate to market value in the predicted manner. Some key findings are: (1) post-retirement assets and obligations are priced as if they are assets and obligations of the company, including some amounts

that are unrecorded, but disclosed in footnotes (Barth, Beaver, and Landsman 1992, 1993); (2) fair values of financial instruments are priced, even if reported only in the footnotes (Barth, Beaver, and Landsman 1996, 1997); (3) various components of earnings are associated with different price multiples, in ways consistent with the degree of measurement error or perceived persistence (Barth, Beaver, and Wolfson 1990); (4) the relative importance of book value and income varies with the financial health of the firm (Barth, Beaver, and Landsman 1998); and (5) cash flows and accruals are priced differently (Barth, Beaver, Hand, and Landsman 1999, 2005).

These two approaches address two different questions. The choice of approach depends on the question the researcher wishes to address. Having been a participant in both types of research, I find that each can be a particularly useful research design, depending on the research question being asked. The approaches are not mutually exclusive.

III. SIX DECADES OF TEACHING

A major portion of our professional life is devoted to teaching and preparing to teach. As I have indicated previously (Beaver 1984, 1987), I believe there is no conflict between research and teaching. In fact, they are mutually supportive; research informs teaching and teaching informs research. How research informs teaching is straightforward. The research on prediction of financial distress, security price behavior, conservatism, and discretionary behavior are prime examples. However, the relation also operates in the opposite direction. For example, my dissatisfaction with how I was teaching foreign currency accounting (Beaver and Wolfson 1982, 1984) and accounting for inflation (Beaver, Christie, and Griffin 1980a; Beaver 1981; Beaver, Griffin, and Landsman 1982, 1983; Beaver and Landsman 1983; Beaver and Ryan 1985) led me to conduct research in these areas. Also, my textbook, *Financial Reporting: An Accounting Revolution* (Beaver 1989), was developed from class notes from a second-year M.B.A. class.[11]

In addition to my M.B.A. teaching, I have worked with a number of outstanding doctoral students—too many to mention explicitly. As I discussed earlier, I began in The University of Chicago M.B.A. program and transferred into the doctoral program. So when I came to Stanford University, I looked to the M.B.A.s as a natural and plentiful source for our doctoral program. I found that it was difficult to convince M.B.A.s to switch careers and join the academic community. However, Pete Dukes and Wayne Landsman are two important conversions from the M.B.A. program. George Foster was an outstanding doctoral student and is an excellent colleague, with an outstanding record of research and teaching.

Some of our best accounting students came from public accounting, including Mary Barth. Stephen Ryan is a prime example of someone who came to us with a strong economics background. All were research assistants and, because of their outstanding contributions, all were coauthors. At last count, we accounted for over 20 coauthored publications. Including all doctoral students, there are over 40 publications.

They have become colleagues, coauthors, and among my closest friends. When I began my career, I thought I would measure success by the list of publications, but I was wrong. The doctoral students with whom I have worked have been the most meaningful aspect of my career.

IV. SIX DECADES OF AAA PARTICIPATION

Participation in the American Accounting Association (AAA) has been a major part of my professional life. Chuck Horngren was a role model and encouraged me as an Assistant Professor to become active in the AAA. One of my earliest committee memberships was a committee on

[11] Beaver (1991) was also based on class notes.

accounting for leases. Some issues never seem to go away. I served on several committees over the years. The awards committees were probably the most fun, because you are essentially giving "good news" to the recipients. I was also honored to serve as Vice President and President. These activities have led to lifelong friendships. I also want to highlight two AAA programs—the Doctoral Consortium and the Distinguished International Lecturer Program.

AAA Doctoral Consortium

In addition to committee work and offices held, I also have participated in the AAA Doctoral Consortium several times, which is one of the most valuable AAA programs. I was fortunate enough to be a faculty member at the first doctoral consortium held at the AAA meetings in Lexington, Kentucky, in 1971. My topic was security price research. Other faculty included Bill Cooper, Sid Davidson, Joel Demski, Yuji Ijiri, Bob Mautz, Bob May, and Fred Skousen, among others. Students included Dan Givoly, Bob Magee, Gary Previtts, and David Wilson, among others. The leveraging effects of this program are enormous. The student participants have an effect on each other, on their fellow doctoral students at their home institution, and on the faculty they ultimately join. Alumni of the doctoral consortia are among the leaders of our profession.

AAA Distinguished International Lecturer

In 1979, I was fortunate to be asked to be the Distinguished International Lecturer. It was an outstanding program and I regret that it has been discontinued. Over six weeks, I spoke at ten universities and delivered 20 lectures. I began in Bergen on May 1 at the Norwegian School of Economics with snowfall, and by early June, I was in Rome with temperatures in the mid-80s. I was honored to receive the invitation, but reluctant to be away from my family for six weeks. The solution was to bring them along; our children were 8, 12, and 13. It was a life-altering experience for all of us.

Our colleagues are more than those at our home academic institution, and I consider the AAA members as my colleagues. The AAA is the accounting academic profession and offers a unique opportunity to interact with your colleagues through AAA publications, meetings, and other activities. My participation has been a rewarding experience. I am indebted to the AAA for having been a major beneficiary of their activities.

V. CURRENT RESEARCH

Since joining the ranks of the emeriti in 1996, I have been able to maintain the veneer of research productivity with the help of outstanding coauthors. With some pride, I had two publications in a recent issue of the *Review of Accounting Studies* (Beaver et al. 2012a, 2012b).

Beaver et al. (2012b) examine the association of financial reporting attributes with the ability of financial ratios to predict bankruptcy. We examine proxies for discretion over financial reporting, the importance of intangible assets, the comprehensiveness of the accounting model, and the recognition of losses. We find that financial ratios are less informative where discretion is greater, where intangible assets are a higher percentage of revenues, where the accounting model is less comprehensive, and where losses are recognized. Beaver et al. (2012a) was discussed earlier.

Currently, I have four active research projects: bankruptcy prediction in an international setting, time-series and cross-section determinants of return volatility at earnings announcements (the U-statistic), costs and benefits of forming portfolios based on accounting-based anomaly

variables, and the effects of the introduction of credit default swaps on sole underwriter participation in loan syndicates and on the interest rates paid by borrowers.

Bankruptcy Prediction in an International Setting

The study is coauthored with Maria Correia and Maureen McNichols. It is an extension of our earlier bankruptcy work. We examine the predictive ability of financial ratios on a large sample of international (non-U.S.) firms. It is important to know to what extent the models developed on U.S. data extend to other countries in the world. Moreover, because the reporting requirements in other countries differ from and, in some respects, are more comprehensive than those of the United States, the sample includes privately held, as well as publicly traded, firms. It also includes financial statements of subsidiaries as well as parents, and denotes whether the financial statements were prepared on a consolidated basis or not.

We are finding that the predictive power of financial ratios is actually slightly stronger for non-U.S. than for U.S. companies; is stronger for parent companies than for subsidiaries; is stronger for publicly traded companies than for privately held companies; and is stronger for companies with consolidated financial statements than for companies whose financial statements are prepared on an unconsolidated basis. This last finding provides support for the longstanding contention in accounting texts, as well as accounting standards pronouncements, that consolidated financial statements provide a more comprehensive portrayal of financial position.

Reexamination of the Information Content of Earnings

Maureen McNichols, Zach Wang, and I are examining the time-series and cross-sectional behavior of information content of earnings announcements. There has been considerable research to address whether the information content of earnings, as measured by the U-statistic, has increased or decreased over time. I always get the "willies" when someone wants to investigate whether the Beaver (1968) result is robust with respect to time. I feel there ought to be a "statute of limitations" on how long I am responsible for the result. Subsequent research has found that the information content of earnings has increased, rather than decreased, over time, notwithstanding the fact that technology has made an enormous amount of data available to investors from a variety of sources at ever-decreasing cost, almost continuously. Prior studies offer differing explanations for the increase.

We are interested in assessing what has happened to the information content of earnings over a more recent time period that includes the Great Recession. On the one hand, there might be a greater premium on more timely information or the driving factor is macro in nature. On the other hand, in times of greater uncertainty, there may be a greater premium on the reliability of information, which is potentially provided by the audited financial statements. We are also examining the U-statistic as a function of size, number of analysts, profitability, and a number of other variables. Using a three-day event window for announcements over the period 1971 to 2011, we are initially finding that the U-statistic is at an all-time high in the last ten years of our study (2001 to 2011). Moreover, the U-statistic on a one-day event window is substantially higher than on a three-day window.

Costs and Benefits of Accounting-Based Anomaly Portfolio Strategies

Maureen McNichols, Richard Price, and I are examining the properties of long-short investment (hedge) strategies used in many academic studies. We incorporate costs of implementing such strategies, particularly those associated with taking short positions. We further

investigate the risk still imbedded in strategies labeled as "hedge" portfolios. We find that: (1) incorporating costs of implementation leads to insignificant net returns for several of the strategies; (2) the net position of a hedge portfolio is still nontrivially risky and certainly not a risk-free position. In other words, these portfolios are not fully diversified, but contain a nontrivial degree of diversifiable risk; (3) the long-short strategies often do not beat a simple investment in the market portfolio, as measured by the Sharpe ratio; and (4) finally, we find that accounting-based anomaly strategies, in combination with each other and in combination with a market portfolio, can produce higher Sharpe ratios than the market. These combinations effectively reduce the diversifiable risk from a pure anomaly-based strategy.

Effects of CDS Initiation on Syndicate Participation and Interest Rates

Wayne Landsman, Dan Amiram, Donny Zhao, and I are investigating the effects of the presence of a credit default swap (CDS) security on loan syndication participation and on yields paid by debt issuers. The basic idea is that the presence of a CDS security permits the leader in the syndicate to lay off some risk that other partners in the syndicate would prefer the leader keep, in order to provide the incentive for the lead underwriter to conduct monitoring and credit evaluation activities. This presence of the CDS market potentially makes it costlier to engage in syndicates. The prediction is that there would be an increase in the frequency of sole lender credit arrangements and an increase in the yield paid by the issuer. Using an accounting-based measure of transparency, our study finds that greater transparency mitigates the sole lender effect.

VI. CONCLUDING REMARKS

My story is not primarily about research papers, but rather about people. I have been fortunate to have met and worked with an outstanding set of colleagues, coauthors, and doctoral students. I have a shared experience at Chicago and Stanford with two colleagues—Chuck Horngren and Joel Demski. Many years ago, on a winter day in February when we had not seen the sun in Chicago for 13 consecutive weekends, Chuck Horngren invited Sue and me to come to Stanford for a faculty interview. The weather in California was sunny and in the 70s. As always, Chuck was a master of timing. The contribution of Chuck Horngren to my career and that of others is enormous and is an entire topic in and of itself.

Joel and I have many stories. He is an outstanding colleague, researcher, teacher, and friend. I have a much better understanding of information economics because of Joel, and that understanding has significantly informed my research. As I remember, one day on our way to the Berkeley-Stanford Accounting Seminar, we got stuck in traffic on the Bay Bridge. With time to kill, we started discussing what Stanford courses or programs we might add. By the time we reached Berkeley, we had sketched the outline of the Stanford Summer Accounting Camp and a fully integrated three-quarter doctoral seminar series. Both have been replicated in the rest of the GSB and across the country.

Many thanks for my family, whose support is responsible for so much of my career. Looking forward, I am as excited about the research opportunities today as I was when I began six decades ago. Superior databases are becoming available daily. Our knowledge based on analytical models of information is continually expanding. The tools at our disposal have never been greater. My advice is to choose good colleagues, choose good students, and choose good coauthors. I have been fortunate to have an abundance of all three. Most of all, remember, "You Can't Always Get What You Want. . .but if you try sometimes, you might find you get what you need."

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Are Fraud Specialists Relatively More Effective than Auditors at Modifying Audit Programs in the Presence of Fraud Risk?

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ABSTRACT: Previous studies indicate that auditors are able to identify fraud risk factors, but may not be able to translate this knowledge into an audit plan that effectively takes these factors into account to increase the likelihood of detecting fraud. Fraud specialists may be able to compensate for such limitations. This study investigates the relative merits of involving fraud specialists in assisting auditors by developing an audit plan that would effectively address fraud risk in a revenue cycle. Results show that fraud specialists did not differ from auditors in the number of procedures selected from a standard audit program; nor were these procedures cumulatively more effective than those selected by auditors. Fraud specialists generated a greater number of non-standard additional audit procedures, and those procedures were marginally more effective, but less efficient, than those of auditors, except for certain groups of procedures. Finally, although the fraud specialists proposed significantly more additional (non-standard) procedures than auditors, their proposed budget increase for this category of procedures was significantly smaller than the budget increase proposed by auditors. Adjustments to the overall time budget did not differ between fraud specialists and auditors.

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I. INTRODUCTION

Since the passage of Statement on Auditing Standards (SAS) 99 (Public Company Accounting Oversight Board [PCAOB] 2008) and the Sarbanes-Oxley Act of 2002 (U.S. House of Representatives 2002), policy-makers and regulators have promulgated additional guidance aimed at improving auditors' performance in assessing and responding to fraud risks of audit clients (PCAOB 2008). Auditors are expected to address fraud risk through the design of their audit methods and programs and by involving specialists (PCAOB 2008; International Auditing and Assurance Standards Board [IAASB] 2010a, 2010b).We study whether fraud specialists are relatively more effective than auditors in designing an audit program that will address elevated fraud risk. Our goal is to examine whether the expertise of fraud specialists can directly contribute to planning the nature and extent of audit procedures, and whether the mix of procedures that such specialists recommend is likely to be more effective and efficient than the procedures proposed by auditors in a setting where *ex ante* fraud risk is rated at above a low level.

Auditing standards (PCAOB 2008, SAS No. 99; IAASB 2010a, ISA 240; IAASB 2010b, ISA 330) have recommended and prior research (e.g., Asare and Wright 2004) has shown that when there is a perceived heightened risk of fraud, auditors are likely to seek the assistance of risk management partners or fraud specialists, such as forensic accountants, in developing an audit plan. However, since consultations with specialists are costly, auditors may avoid referring issues to them unless they believe that a substantially elevated risk of fraud is present in an engagement, especially when a requirement for such consultation is not a strict one (Gold, Knechel, and Wallage 2012). In such a setting, auditors may convince themselves that the fraud risk in an engagement is lower than it really is. Alternatively, auditors may erroneously assume that consulting with fraud specialists would safeguard them against audit program design weaknesses when, in fact, some fraud specialists may have limited competence in audit program design (American Institute of Certified Public Accountants [AICPA] 2006; DeZoort and Stanley 2006; Meservy, Romney, and Zimbelman 2006). By directly examining fraud specialists' recommendations for an audit plan under conditions of an elevated fraud risk, we seek to clarify whether there are benefits in requesting fraud specialists to participate in audit program design.[1]

We test four hypotheses using an experimental approach. First, we examine whether, under conditions of elevated[2] fraud risk, fraud specialists select more procedures from a standard audit program for a revenue cycle than auditors, and whether the procedures selected by fraud specialists from a standard program are more effective. Second, we examine whether fraud specialists develop more additional (non-standard) procedures than regular financial statement auditors. Third, we study differences in the effectiveness and efficiency of additional procedures proposed by fraud specialists and financial statement auditors by comparing those procedures against a benchmark based upon the judgments of an expert panel of auditors. Fourth, we examine whether fraud specialists modify the audit time budget for a revenue cycle differently from financial statement auditors.

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[1] In this study, the judgments of fraud specialists are compared to the judgments of audit partners and managers, i.e., our fraud specialists have more extensive audit experience than audit seniors. Audit seniors have been shown to have limited fraud risk assessment and response capabilities (see Hammersley, Johnstone, and Kadous 2011).

[2] We use the terms "elevated" or "heightened" to denote the fact that the risk was higher than low (5.1 on a scale from 1 to 10), but not definitive in the sense that fraud was presumed to exist. In other words, the participants' task involved planning an effective and efficient audit program, *not* a fraud investigation.

The experimental case was presented to the fraud specialists and to the financial statement auditors under the scenario that the audit team has assessed the client as having a "higher than low" risk of fraud, and that the participants' assistance is sought to assess the adequacy of the audit program and to change it as necessary. Similar to Hoffman and Zimbelman (2009), we do not manipulate fraud risk as a factor in our experiment because our goal is to investigate the proposed changes to the audit program given a heightened risk of fraud. The participants were provided with a copy of a standard audit program adapted from Asare and Wright (2004) and asked to identify which procedures in the standard audit program were needed. They were also asked to supplement the standard program with any additional procedures that were required, in their opinion, to address the elevated risk of fraud in the revenue cycle. Finally, they were asked about the amount of time that should be budgeted for standard and additional procedures, as compared to the previous year.

Our results reveal the following three insights. First, in a situation with elevated fraud risk, fraud specialists did not select more procedures from a standard audit program than financial statement auditors; nor were the selected procedures more effective than those selected by auditors. Second, the fraud specialists proposed more additional procedures than did auditors, and the specialist-proposed additional procedures were marginally more effective, but significantly less efficient, than the additional procedures proposed by auditors. Third, fraud specialists increased time budgets to reflect the additional effort that they proposed via extensive non-standard procedures. However, although they proposed significantly more additional procedures than did auditors, their proposed time budget increases for those procedures were significantly lower than adjustments proposed by the auditors.

The next section reviews relevant literature and develops our hypotheses. Section III describes our methodology, and Section IV presents the results of the analysis of the experimental data. Section V discusses conclusions, limitations, and avenues for future research.

II. LITERATURE REVIEW AND HYPOTHESES

Fraud Risk Assessment and Audit Planning

Audit research has addressed fraud risk assessment and associated planning decisions. Nieschwietz, Schultz, and Zimbelman (2000) and Hammersley (2011) provide detailed reviews of the literature on auditor judgment in fraud-related tasks. While earlier studies examined whether auditors incorporate information about fraud risk factors in their fraud risk assessments (e.g., Pincus 1989; Loebbecke, Eining, and Willingham 1989; Hackenbrack 1993; Heiman-Hoffman, Morgan, and Patton 1996), more recent studies have focused on whether information about fraud risk factors and associated risk assessments is reflected in auditors' decisions about the nature, timing, and extent of audit tests. These studies have shown that auditors respond to a high risk of fraud at a client primarily by changing the extent of the procedures and increasing the overall audit investment (e.g., Zimbelman 1997; Houston, Peters, and Pratt 1999; Glover, Prawitt, Schultz, and Zimbelman 2003; Mock and Turner 2005) and, to some degree, by modifying the nature of the proposed audit tests (e.g., Wright and Bédard 2000; Graham and Bédard 2003; Hammersley et al. 2011). This research has established that auditors respond to audit risk factors with an increased assessed risk of fraud, and that they are able to recognize the need for modifying the audit program in response to such risk (Cormier and Lapointe-Antunes 2006; Hammersley 2011), but are unable to design procedures that are effective and efficient at identifying fraud (Hammersley et al. 2011). PCAOB inspectors have also observed that auditors "failed to expand audit procedures when addressing identified fraud risk factors" (PCAOB 2007, 3) and failed to use the fraud risk assessment process "with a view toward identifying ways to modify the audit plan in order to address the [fraud] risk" (PCAOB 2007, 4).

As the risk of fraud and associated losses to business organizations have been projected to rise (e.g., KPMG 2009; DeMarco 2010), researchers have investigated specific techniques and approaches that may increase auditors' effectiveness in fraud risk assessment and the quality of the corresponding adjustments to the audit program. Most notably, brainstorming techniques have shown good results in stimulating auditors' ability to develop quality ideas about potential fraud in a client, to incorporate them in fraud risk assessments (Carpenter 2007; Brazel, Carpenter, and Jenkins 2010), and to identify specific modifications to an audit program at a client where fraud is suspected (Brazel et al. 2010; Hoffman and Zimbelman 2009). The use of strategic reasoning (Wilks and Zimbelman 2004; Hoffman and Zimbelman 2009), priming auditors to reconsider the important fraud risks documented during planning before they start evidence evaluation, and use of specific fraud risk memos that include, along with a summary, detailed fraud risks discussed during the brainstorming session (Hammersley, Bamber, and Carpenter 2010) have shown potential as methods of making auditors more effective in addressing fraud risk during the audit planning and evidence evaluation process. Finally, research has documented that auditors recognize situations where their expertise alone is not sufficient in addressing the risk of fraud and that they are willing to consult with a technical expert, such as a risk management partner (Asare and Wright 2004).

Since financial statement auditors are unable to design effective audit plans to address fraud risk, a potential response to heightened risk of fraud is to involve fraud specialists. The propensity to consult with fraud specialists tends to increase when firm-level guidance makes consultations mandatory and binding, particularly under conditions of high fraud risk and tight deadline pressure (Gold et al. 2012). However, to our knowledge, there is no published research that provides systematic empirical evidence (either archival or experimental) that fraud specialists design audit plans differently from auditors. Asare and Wright (2004) consider their participants' referrals to risk management partners as a positive outcome on the assumption that they are better than regular auditors at designing audit plans that effectively address fraud risks. In contrast, Jamal (2008, 103) suggests that forensic accountants may not be able to respond to fraud risk better, in the context of an assurance engagement, than financial statement auditors because their expertise may reside in "doing detailed *ex post* data collection and evidence documentation work required to make a case for court." In other words, forensic specialists may be able to investigate a suspected fraud, but their expertise may not be suitable for designing *audit tests to respond to fraud risk effectively and efficiently in the context of planning an assurance engagement.*

Fraud Specialists' Expertise

The usefulness of consultations with fraud specialists, such as forensic accountants, is based on the premise that expertise in fraud investigation is directly transferable to the audit setting. In other words, it is assumed that the knowledge, skills, and attitudes that fraud specialists bring to the task of audit planning under conditions of elevated fraud risk are more relevant to the specific task than the knowledge, skill, and attitudes possessed by financial statement auditors who are not trained in financial forensics or fraud investigation. Research on audit expertise (Libby 1995, 178–188) and the effects of knowledge on judgment quality (Bonner 2008, 66–70) suggests that *if* fraud specialists' knowledge and skills are a better match for the specific task of audit program design under conditions of "higher than low" fraud risk than those possessed by auditors, then specialists will outperform auditors on that task. However, academic research has not yet addressed this conjecture from either the theoretical (i.e., the analysis of expertise required for the task) or the empirical points of view. There are studies examining opportunities for forensic auditors (Beasley 2003; DeZoort and Stanley 2006), curriculum and certification requirements for forensic accountants and auditors (Rezaee 2002; Meservy et al. 2006), and forensic experts' classification of fraud risk factors (Apostolou, Hassell, and Weber 2000; Hansen and Klamm 2004). None of

these studies focus on whether fraud specialists' expertise is suitable for the audit-planning task and, as a result, whether their judgments can be effective in an audit-planning context. Hammersley (2011) and Hammersley et al. (2011) note that special characteristics of the financial statement fraud setting and auditors' limited expertise in making fraud-related judgments make it a challenging setting for audit program planning; however, these studies focus on judgments by regular financial statement auditors rather than fraud specialists.

It would be reasonable to expect that, if consulted, fraud specialists with prior auditing training are likely to have sufficient knowledge that would enable them to recommend procedures that would effectively address the risk of fraud in the context of an audit engagement (Association of Certified Fraud Examiners [ACFE] 2010; AICPA 2006, 2010; Canadian Institute of Chartered Accountants [CICA] 2010; IFA Alliance for Excellence in Investigative and Forensic Accounting [IFA Alliance] 2010). In contrast, financial statement auditors are able, generally speaking, to design effective audit programs, but lack the knowledge and skill to design fraud discovery procedures (Houston et al. 1999) or modify the existing audit program well enough to address the risk of fraud (Hammersley et al. 2011). This lack of requisite expertise in fraud investigation would limit their performance in planning an audit under conditions of elevated fraud risk, as compared to fraud specialists with prior audit training.

In addition to knowledge and skills, there is an attitudinal component of the expertise of fraud specialists that influences the quality of fraud risk judgments—professional skepticism. When auditors are disciplined for their failure to detect the financial statement fraud of a client, 60 percent of the time, it is found that they did not exercise sufficient professional skepticism (Beasley, Carcello, and Hermanson 2001). Nelson (2009) relates professional skepticism to both experience and specialization, and notes that specialists and auditors with domain-specific experience are more likely to identify risks of domain-specific errors and modify their audit-planning decisions accordingly. We infer that Nelson's (2009) conclusion applies to fraud specialists and that the increased skepticism on the part of a fraud specialist, combined with his or her knowledge and skill derived from training and experience, *ceteris paribus,* will lead to better recognition of fraud risks (Hammersley et al. 2011), followed by the selection of more effective and efficient procedures from a standard audit program, as well as identification of non-standard procedures that will address the risk of fraud.

Use of Specialists

Although reliance on specialists may improve the effectiveness of an audit, this potential improvement comes at a cost. As noted by Hasseldine, Holland, and van der Rijt (2011), the very skepticism that can lead to positive planning outcomes can make specialist services unattractive to engagement partners, who may fear that excessive skepticism may needlessly inflate audit costs. Therefore, unless the improvements are significant, engagement partners may not avail themselves of the services of specialists due to cost-benefit considerations. For example, a report by the Canadian Public Accountability Board (CPAB 2009, 16) notes that "some partners appeared to be reluctant to use specialists in difficult areas." Johnstone and Bedard (2003) find that the intent to use specialist personnel (i.e., personnel with relatively high industry expertise, functional expertise, and/or high-risk client experience) moderates the negative relationship between audit risk (i.e., risk of misstatement due to fraud or error) and the likelihood of client acceptance. They call for research on the effects of specialization on audit quality that systematically investigates specialists' application of their knowledge across audit tasks, focusing on where gains from the use of specialists are most likely to occur, how those gains can be leveraged by firms, and what factors might inhibit those gains (Johnstone and Bedard 2003, 1022).

There are no published studies involving fraud specialists in an audit-planning context. However, several published studies in auditing have looked into use of computer information

technology (IT) specialists in audit engagements. Brazel and Agoglia (2007) demonstrated that auditors with high accounting information systems expertise were able to effectively incorporate, and even compensate for, deficiencies in recommendations of computer assurance specialists in control risk assessments and planning of control tests within a complex accounting systems environment. However, in their study, the recommendations of computer assurance specialists were manipulated and provided to the participant auditors, rather than elicited directly from the specialists. The use of IT specialists is often less frequent than expected (Janvrin, Bierstaker, and Lowe 2008, 2009). Similarly, Hasseldine et al. (2011) observe that some potential users of tax specialists are reluctant to avail themselves of their services out of a concern that specialists' propensity to focus (excessively) on risks can lead to implementation of costly risk-mitigating measures that might not be necessary. Such behavior can create concerns for auditors seeking to stay within budgeted audit fee levels, especially under the current conditions of downward fee pressure (McCann 2010; Reason 2010). Finally, the effectiveness of specialists' services cannot simply be assumed. For example, in its inspection report, the CPAB (2009, 16) noted that "the work of some [valuation] specialists lacked rigor."

Fraud specialists are not routinely employed to help design or adjust audit programs in situations of elevated risk. Brazel et al. (2010) find that a fraud specialist attends a primary brainstorming/planning session in only 31 percent (Std. Dev. = 46.34) of audit engagements in their sample (Brazel et al. 2010, 1284, Table 3). Fraud specialists are not necessarily trained for, and do not typically work on, financial statement audit-planning tasks where fraud has not yet been identified or fraud risk is assessed at a low level. Instead, they focus on evaluating situations where a fraud has already been identified; the job of the fraud specialists is to determine the magnitude of the problem. These are different tasks with different goals. As such, unless the use of a specialist can be shown to clearly contribute to the more effective and efficient delivery of professional services, financial statement auditors are likely to underutilize such specialists. This underutilization can be particularly problematic in a fraud risk context since, unlike the technical risks addressed by IT and tax specialists, fraud risks relate to strategic actions taken by client personnel to perpetrate and conceal wrongdoing. Audit responses to such actions may require insights from both auditors *and* specialists in order to guide the design of effective and efficient audit plans, especially when fraud risks are elevated, but not to the level that clearly calls for a fraud investigation.

Hypotheses

Based on the above discussion, we predict that the fraud-related knowledge, skills, and skepticism of fraud specialists will influence their program-planning judgments and will lead to greater effort in addressing fraud risk, such as selection of more (and more effective) procedures from a standard audit program, as well as identification of more (and more effective) non-standard procedures, as compared to financial statement auditors. We examine modifications proposed to the standard audit program separately from the proposed non-standard additional procedures because the previous research has distinguished these planning decisions as two distinct sets of judgments (e.g., Asare and Wright 2004). Indeed, the expertise paradigm would suggest that selecting from a list of standard procedures is a cognitively different task than developing an unaided list of additional, non-standard procedures. Fraud specialists' expertise is likely to apply more directly to the latter, and less so to the former.

We expect that fraud specialists will do the following: (1) increase overall audit effort by proposing greater numbers of both standard and additional (non-standard) audit procedures; (2) select more effective audit procedures from a standard program when the risk of fraud is higher than low; (3) propose more effective additional, non-standard procedures than those proposed by financial statement auditors; and, as a corollary to (1), (2), and (3), (4) increase the budgeted hours allocated to all (standard and additional) procedures in the audit program. Stated formally:

H1a: When risk of fraud is other than low, fraud specialists will select more procedures from a standard audit program than will auditors.

H1b: The standard procedures selected by fraud specialists will be more *effective,* as judged by audit experts, than those selected by auditors.

H2a: When risk of fraud is other than low, fraud specialists will propose more additional procedures than will auditors.

H2b: The additional procedures proposed by fraud specialists will be more *effective,* as judged by audit experts, than the procedures proposed by auditors.

When fraud specialists are consulted for program-planning advice, they may feel less constrained by the budget than the audit team. As a result, fraud specialists may identify procedures that, while effective, may not be viewed as efficient by the engagement partner or the audit team members (Jamal 2008). This could be due to the fact that a typical audit engagement is much more budget-driven than a typical forensic engagement, as described to us by partners from Big 4 forensic and assurance practices in Canada and in the U.S., and from a specialty forensic firm in Canada. In other words, fraud specialists may propose procedures that would address fraud risk successfully, but, in the eyes of the engagement partner, may be too costly (i.e., inefficient). Therefore, our third hypothesis states:

H3: When risk of fraud is other than low, fraud specialists will propose additional procedures that are less *efficient,* as judged by audit experts, than the additional procedures proposed by auditors.

While individual procedures may be effective and/or efficient, a combination of several such procedures, when added to the audit program, may result in a significant increase in the budgeted hours for the engagement. If H1a and H2a are supported, then fraud specialists should increase the budgeted hours for the engagement to a greater extent than auditors, as we anticipate that they are likely to suggest conducting a greater number of standard and extended procedures than auditors. Therefore, our fourth hypothesis states:

H4: When risk of fraud is other than low, fraud specialists will increase the budgeted hours allocated to the procedures in a standard audit program and additional procedures to a greater extent than will auditors.

III. METHODOLOGY

Experimental Design and Task

The experimental design includes one between-participants factor, *Participant Type* (fraud specialists versus financial statement auditors). Fraud specialists and auditors participated in an audit-planning task by selecting procedures from a standard audit program for the revenue cycle, followed by proposing additional, extended procedures and revising the associated time budget.[3]

[3] We also attempted to manipulate another factor (i.e., mode of involvement in audit planning at two levels: participative versus consultative) between subjects in the fraud specialist subsample. Thus, we administered two versions of the case: one where participants made their own risk assessments (participative mode), and another where the risk assessments were provided to them (consultative mode). However, our manipulation was either (1) not sufficiently strong to produce meaningful results, or (2) resulted in no significant differences in fraud specialists' selections of procedures compared with those of auditors. Since there were no differences between those conditions with respect to the main dependent variables in the study, these conditions were collapsed into one.

Participants completed an audit case that was adapted from Asare and Wright (2004) and was based on an actual company that had issued fraudulent financial statements. The company in the case is a manufacturing firm and the audit of the revenue cycle requires no industry-specific training. The information in the case indicated fraud risk as being higher than low, but not extremely high; that is, there was no direct indication in the case that fraud had been discovered.

First, participants were presented with comparative financial statements and information about the company. Next, they were asked to help in designing an audit program for the revenue cycle by (1) selecting procedures from a standard audit program, (2) proposing additional procedures, and (3) modifying as necessary the budgeted hours for procedure categories. In the final part of the experimental instrument, participants were asked to provide demographic information.

Packages containing sets of case materials in individual envelopes were distributed by mail to contacts at the firms who agreed to participate in the study. The contacts distributed the cases to fraud specialists (or auditors) in their firm. Once the cases were completed, they were returned by the participants directly to the researchers in a self-addressed, postage-paid envelope. Standard tests for early versus late respondents revealed no differences in key variables of the study.

Participants

Thirty-two fraud specialists and 16 auditors completed the case. Half of the fraud specialists were from Big 4 accounting firms and half were from medium-size firms. Sixty percent of the auditors were from Big 4 firms and 40 percent were from medium-size firms. The participants in both groups were partners, directors, or managers with a significant amount of practical experience. On average, the fraud specialists were 41 years old, had 12 years of specialized fraud-related experience, and six years of auditing experience. The auditors were, on average, 36 years old, had 13.25 years of auditing experience, but no specialized fraud experience. Thus, on average, auditors had significantly greater auditing experience (13.25 versus 6.02 years, $p = 0.002$) than fraud specialists; however, fraud specialists had significantly greater specialized fraud experience (12.06 years versus 0.00, $p < 0.001$). Nevertheless, fraud specialists' average audit experience was substantial (6.02 years). Twenty-one of the fraud-specialist participants had an IFA (Investigative Forensic Accountant) specialist designation; some also had a CPA and/or CFE (Certified Fraud Examiner) designation. All IFA-designated fraud specialists were also Chartered Accountants (CAs). On average, the fraud specialists spent about 52 percent of their time on fraud-related activities and 37 percent on litigation-related activities. The auditors spent, on average, 89 percent of their time on audit-related activities. Thus, we conclude that both the auditor and fraud-specialist samples were qualified and experienced in their respective domains and were appropriate participants for the experimental task.

Dependent Variables

Measures of the dependent variable, the quality of the audit program for the revenue cycle, include the following: (1) the number of procedures selected from a standard audit program for the revenue cycle, in total and by type of procedure (i.e., tests of the aged trial balance, confirmations, adequacy of bad debt provisions, cut-off, and analytical procedures); (2) the effectiveness of procedures selected from the standard program; (3) the number of additional procedures proposed to supplement the standard audit program; (4) the effectiveness of additional participant-proposed procedures; (5) the efficiency of additional procedures; and (6) the proposed revisions to the time budget for the final audit program for the entire revenue cycle and by type of procedure.

We invited two groups of audit experts, each consisting of ten audit partners and senior managers, to serve as our expert panels. We asked the first panel to evaluate the effectiveness and

efficiency of the procedures in the standard program, and the second panel to evaluate the effectiveness and efficiency of the additional procedures recommended by auditors and specialists. We invited audit experts, rather than forensic or fraud experts, to rate effectiveness and efficiency of procedures because our discussions with audit partners from the Big 4 assurance practices in the U.S. and Canada indicated that the ultimate decision about the use of fraud specialist-recommended procedures on an audit engagement lies with the engagement partner. Hence, we believe that it is appropriate to use expert panels of experienced *auditors* (as opposed to partners in forensic practice or forensic specialty firms) in judging the quality (effectiveness, efficiency) of an *audit* program. The audit experts on both panels were partners and senior managers from two Big 4 (six experts on the first, and four experts on the second panel) and the two next-largest international firms (Grant Thornton and BDO Dunwoody) (four and six, respectively). Four (six) of them were male and six (four) were female. Their average age was 39 (42), and they had, on average, 17 (18) years of audit experience. We conclude that both of our panels of experts had the expertise required to evaluate the effectiveness and efficiency of audit procedures, and they were very comparable to each other in terms of experience, rank in the firm, gender, and age (all p-values > 0.10).

The audit expert panels were not involved in any of the foregoing phases of the study. They were given the same case information as the auditors and fraud specialists. They were not told that a fraud had actually occurred in the case company. The first panel was given a standard audit program for the revenue cycle and was asked to assess the effectiveness and efficiency of each procedure in this program. This was done using a scale from 0 to 10, where 0 meant "totally ineffective (inefficient)" and 10 meant "extremely effective (efficient)." The second expert panel was given a standard audit program (but not asked to evaluate it), as well as a list of the 65 additional (non-standard, extended) procedures that participants had proposed during the main experiment. These procedures were presented to the experts as additional, extended procedures that members of the audit team had generated as a part of their planning process. The audit experts were asked to review the additional procedures and assess the effectiveness (efficiency) of each procedure on a scale from 0 to 10, where 0 meant "totally ineffective (inefficient)" and 10 meant "extremely effective (efficient)." We did not specifically define the terms "effectiveness" and "efficiency" for the panels, but expected them to apply the common understanding of these terms as they are used in auditing; that is, "effectiveness" of an audit procedure is its ability to achieve an audit objective, regardless of the cost, and "efficiency" of an audit procedure is the achievement of an audit objective without undue or excessive cost. Effectiveness takes precedence over efficiency, since there would be little value in conducting a procedure that was efficient, but ineffective. However, at a satisfactory level of effectiveness, various procedures may possess different degrees of efficiency.

We used experts' average ratings of effectiveness and efficiency of each standard procedure (first panel) and each additional procedure (second panel) as "weights" to develop overall effectiveness and overall efficiency scores for standard procedure selections and for additional procedure listings, respectively. We also analyzed the overall quality of an audit procedure, defined as the combination of its effectiveness and efficiency.

In our analyses, we control for audit experience to separate the effect of audit experience from fraud experience. As Table 1 shows, fraud specialists in our sample had an average of about six years of audit experience, whereas the auditors had an average of 13 years of audit experience. The audit experience of the auditors may compensate, to a degree, for their lack of specialized fraud experience and may make it difficult to tease out the contribution of the fraud specialists to the audit program plan. Therefore, as we test each hypothesis, we provide additional analyses with audit experience as a covariate.

TABLE 1

Participant Demographic Data

Demographic Variable	Fraud Specialists (n = 31)[a]			Auditors (n = 16)		
	Mean/Count (Standard Deviation/Percentage of the Sample)					
Age	41.29	(8.25)	years	35.81	(6.56)	years
Gender	21	(67.7%)	male	7	(43.8%)	male
Years of Specialized Fraud Experience	12.06	(7.50)	years	0.00	(0.00)	years
Years of Auditing Experience[b]	6.20	(7.00)	years	13.25	(6.93)	years
Current Position	12	(38.7%)	partners	5	(31.3%)	partners
	16	(51.6%)	senior managers/ directors	6	(37.5%)	senior managers
	3	(9.7%)	managers	4	(25%)	managers
				1	(6.3%)	principal
CA designation	26	(83.9%)	yes	16	(100%)	yes
	5	(16.1%)	no	0	(0%)	no
CPA designation	3	(9.7 %)	yes	3	(18.8%)	yes
	28	(90.3%)	no	13	(81.3%)	no
CFE designation[c]	10	(32.3%)	yes	0	(0%)	yes
	21	(67.7%)	no	16	(100%)	no
IFA designation[c]	21	(67.7%)	yes	0	(0%)	yes
	10	(32.3%)	no	16	(100%)	no
Number of times contacted for [sought] consultation by audit group [of fraud specialists] during the last five years[d]	4.69	(9.49)		0.12	(0.00)	

[a] 31 out of 32 fraud specialists provided demographic information.
[b] Range of audit experience was from 0 minimum to 36 maximum years for fraud specialists, and from 4 minimum to 25 maximum for auditors; range of specialized fraud experience was from 4 minimum to 30 maximum years for fraud specialists.
[c] CFE = Certified Fraud Examiner (ACFE 2010); IFA = Investigative Forensic Accountant (IFA Alliance 2010).
[d] Range was from 0 minimum (11 participants) to 45 (one participant) times for fraud specialists, and from 0 minimum (15 participants) to 2 maximum (one participant) for auditors.

IV. ANALYSIS AND RESULTS

Tests of H1

H1a predicts that fraud specialists will select a greater number of procedures from the standard audit program for the revenue cycle than financial statement auditors. We test this hypothesis by comparing the number of procedures selected from a standard audit program for the revenue cycle by fraud specialists and auditors. When we compare the number of standard procedures selected in total, we find no statistically significant difference between the number of procedures selected by fraud specialists and auditors (Table 2, Panel A: means 14.28 versus 13.81, respectively, $p = 0.422$, one-tailed). When we examine differences in procedure selection by groups of procedures, we find no significant differences (Table 2, Panel A). Thus, we conclude that H1a is not supported.

H1b predicts that the standard procedures selected by fraud specialists will be more effective than those selected by auditors. To test H1b, we use the expert panel's ratings of standard procedure

TABLE 2

Standard Audit Program Selections
Descriptive Statistics

Panel A: Number of Standard Procedures Selected

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
Number of Standard Procedures Selected for:			
tests of aged trial balance (out of 3)	2.06 (1.16)	1.81 (1.28)	0.250
confirmations (out of 6)	4.28 (2.36)	4.56 (2.34)	0.349
tests of adequacy of bad debt provision (out of 4)	3.50 (1.55)	2.25 (1.39)	0.294
cut off tests (out of 2)	1.47 (0.80)	1.63 (0.81)	0.265
analytical procedures (out of 8)	3.97 (2.44)	3.56 (2.07)	0.286
Total number of procedures selected from standard program (out of 23)	14.28 (7.90)	13.81 (7.30)	0.422

Panel B: Proposed Revisions to the Time Budget for the Final Audit Program

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
Revisions in % relative to original budget for:			
tests of aged trial balance	1.00 (3.46)	−5.00 (18.71)	**0.042**
confirmations	4.63 (5.52)	3.13 (5.44)	0.189
tests of adequacy of bad debt provision	4.88 (4.61)	5.00 (7.53)	0.472
cut-off tests	6.53 (6.11)	1.56 (3.01)	**0.002**
analytical procedures	4.28 (5.37)	3.13 (13.89)	0.340
other tests	6.88 (10.61)	23.12 (32.24)	**0.006**
Total revisions to the time budget for the final audit program, in % relative to original budget for all tests	18.50 (15.68)	20.44 (19.44)	0.356

All p-values are based on a t-test and are one-tailed given the directional nature of the hypotheses. The p-values in bold are equal to or below 0.05.

effectiveness and efficiency to compare auditors' and fraud specialists' selections from a standard program. Appendix A provides the proportions of participants who selected individual procedures from the standard audit program for the revenue cycle, and our first expert panel's ratings of the effectiveness and efficiency of those procedures. We create cumulative effectiveness scores for participants' selections from the standard program by group of procedures, and for the entire standard program, using average ratings provided by the first panel of audit experts as effectiveness "weights" for each standard procedure (see the second column of Appendix A). For instance, a cumulative effectiveness score for the "tests of aged trial balance" group of standard procedures is calculated as 5.6 × (0 or 1, depending on whether a participant selected procedure "a" from this group) + 6.9 × (0 or 1, depending on whether a participant selected procedure "b" from this group) + 6.2 × (0 or 1, depending on whether a participant selected procedure "c" from this group). If the participant selected procedures "a" and "b," but not "c," then his or her effectiveness score for this category would be $5.6 \times 1 + 6.9 \times 1 + 6.2 \times 0 = 12.5$; if he or she selected all three, then his or her score would be $5.6 \times 1 + 6.9 \times 1 + 6.2 \times 1 = 18.7$. We perform this calculation for each participant and for each group of standard procedures. Descriptive statistics for these scores, as well as for effectiveness scores for all selections from the standard program, are presented in Table 3. We perform the same type of calculation for efficiency scores for the same groups of procedures and for overall selections from the standard program (see Table 3 and Appendix A).

Fraud specialists show an average effectiveness score of 104.06 (Std. Dev. = 57.18) as compared to auditors with a mean of 99.06 (Std. Dev. = 51.71), but this difference is not statistically significant using a t-test ($p > 0.10$) (Table 3, Panel A). Efficiency scores for the standard program show a similar pattern, with means of 100.31 (Std. Dev. = 55.37) and 94.22 (Std. Dev. = 49.22) for fraud specialists and auditors, respectively ($p > 0.10$). When we examine effectiveness scores by category of procedures, we do not find significant differences, based on a series of t-tests, between fraud specialists and auditors in effectiveness of standard procedures in individual categories (Table 3, Panel B: all p-values > 0.10). Similarly, the efficiency scores of standard procedures, when analyzed by category, are not significantly different between fraud specialists and auditors (Table 3, Panel C: all p-values > 0.10).

The individual t-tests reported in Table 3 do not take into account the potential inter-dependencies between the effectiveness and efficiency of procedures selected in the standard program.[4] Thus, we perform MANCOVAs with standard program effectiveness and efficiency scores as dependent variables and *Participant Type* (fraud specialists versus auditors) as the independent variable, and the number of standard procedures selected from the standard program and/or audit experience as a covariate. *Participant Type* is significant: $F(2,44) = 5.505$, $p = 0.007$ when we control for the number of procedures selected from the standard program; $F(2,42) = 6.414$, $p = 0.004$ when we control for audit experience; and $F(2,41) = 6.552$, $p = 0.003$ when we control for both (not tabulated).

We perform further analyses whereby we include audit experience and the total number of procedures selected from the standard program as covariates in a series of ANCOVAs with overall effectiveness of audit procedures selected by participants from a standard program as a dependent variable. These analyses do not include efficiency as a covariate because, strictly speaking, in order for the procedure to be effective, it does not need to be efficient. When we control for the total number of standard procedures selected and audit experience, we find that at the standard program level, fraud specialists did not select more effective procedures than auditors ($F(1,42) = 0.347$, $p = 0.559$, not tabulated); marginal means for standard program effectiveness score are 100.686 for auditors and 99.954 for fraud specialists (not tabulated). This finding indicates lack of support for H1b.

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[4] Indeed, overall effectiveness and efficiency scores of standard procedures are highly correlated both in our participant sample (0.984, $p < 0.001$, two-tailed) and in the expert panel (0.786, $p < 0.001$, two-tailed).

TABLE 3

Effectiveness and Efficiency of Audit Procedures Selected by Participants from a Standard Program

Panel A: Overall Scores

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
Overall Effectiveness Score of Procedures Selected from the Standard Program	104.06 (57.18)	99.06 (51.71)	0.385
Overall Efficiency Score of Procedures Selected from the Standard Program	100.31 (55.37)	94.22 (49.22)	0.356

Panel B: Effectiveness Scores by Group of Procedures in the Standard Program

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
1. Tests of aged trial balance	12.98 (7.22)	11.42 (7.91)	0.250
2. Confirmations	33.03 (18.05)	35.19 (18.01)	0.349
3. Tests of adequacy of bad debt allowance	18.52 (11.26)	16.88 (10.21)	0.313
4. Cut-off tests	11.24 (6.14)	12.43 (6.17)	0.266
5. Analytical procedures	28.29 (17.03)	23.14 (12.91)	0.146

Panel C: Efficiency Scores by Group of Procedures in the Standard Program

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
1. Tests of aged trial balance	12.95 (7.25)	11.54 (7.90)	0.270
2. Confirmations	29.45 (16.15)	31.41 (16.10)	0.347
3. Tests of adequacy of bad debt allowance	16.30 (9.68)	15.19 (8.89)	0.352
4. Cut-off tests	10.22 (5.57)	11.29 (5.60)	0.267

(continued on next page)

TABLE 3 (continued)

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
5. Analytical procedures	31.39 (19.21)	24.79 (14.05)	0.115

All p-values are based on a t-test and are one-tailed given the directional nature of H1b.

Tests of H2

H2a and H2b predict that when risk of fraud is other than low, fraud specialists will propose more additional procedures that are also more effective at detecting fraud than the additional procedures proposed by auditors. We test these hypotheses in two ways. First, we compare the additional procedures suggested by auditors against those suggested by fraud specialists. Appendix B summarizes 65 additional procedures that one or more of our participants proposed to add to the standard audit program. This list includes procedures that were suggested by Asare and Wright (2004, Exhibit 3, based on the description of a fraud in a 1998 Securities and Exchange Commission [SEC] Accounting and Auditing Enforcement Release). The additional proposed procedures fall into the following seven areas: (1) review of contracts, (2) revenue recognition, (3) confirmations, (4) inventory/warehouse, (5) allowance for bad debts, (6) cut-off tests, and (7) analytical procedures. Appendix B, Panel A indicates that fraud specialists, as a group, proposed a greater variety of additional procedures than did auditors, selecting 60 out of 65 procedures on the list, whereas auditors, as a group, proposed only 22 out of 65 procedures on the list. Across all categories and types of procedures, a greater proportion of fraud specialists proposed additional procedures $\chi^2(10, n = 48) = 13.885$, $p = 0.027$, one-tailed). Panel B of Appendix B shows that 29 fraud specialists (91 percent of the sample) proposed between one and 18 additional procedures, whereas 15 (94 percent of the sample) auditors proposed between one and five additional procedures. The average number of additional non-redundant procedures proposed by fraud specialists was 4.47 (Std. Dev. = 3.95), whereas for auditors, it was 2.75 (Std. Dev. = 1.44), and this difference was statistically significant ($p = 0.05$, one-tailed). Thus, we find support for H2a. Fraud specialists proposed a greater number of additional procedures, on average, than did auditors.

Similarly to the calculation of effectiveness and efficiency scores for standard procedures described previously, we create cumulative scores for the effectiveness of additional procedures by category for each participant (based on the categories in Appendix B, Panel A) using the average ratings provided by the second panel of audit experts as "weights" for each procedure (see the third column of Appendix B, Panel A). We perform this calculation for each participant and for each of the seven additional procedure categories. Finally, we calculate overall effectiveness scores for *all* additional procedures proposed by each participant by summing category-level scores. The scores for efficiency of additional procedures are calculated in a similar way using corresponding average ratings from the fourth column in Appendix B, Panel A.

Descriptive statistics for these scores are reported in Table 4. An examination of mean overall effectiveness scores using a t-test indicates a marginally statistically significant difference between fraud specialists and auditors (Table 4, Panel A: $p = 0.074$, one-tailed). This provides a preliminary indication that the overall effectiveness of additional procedures listed by fraud specialists is greater than that for auditors (means are 26.84 for fraud specialists versus 18.39 for auditors).

TABLE 4

Effectiveness and Efficiency of Participant-Proposed Additional Procedures

Panel A: Overall Scores

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
Overall Effectiveness Score of Additional Procedures	26.84 (21.62)	18.39 (10.32)	*0.074*
Overall Efficiency Score of Additional Procedures	26.96 (24.68)	18.28 (10.12)	*0.093*

Panel B: Effectiveness Scores by Procedure Category

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
1. Procedures to understand the marketing program and review contracts	11.82 (9.48)	7.06 (5.64)	**0.036**
2. Procedures to test revenue recognition.	3.11 (4.52)	4.94 (4.90)	*0.103*
3. Confirmations	1.05 (2.26)	0.00 (0.00)	**0.035**
4. Inventory-related procedures	2.64 (3.85)	1.29 (2.37)	*0.102*
5. Procedures to test allowance for bad debts	3.52 (3.46)	3.30 (4.45)	0.425
6. Cut-off tests	0.89 (3.27)	0.00 (0.00)	0.142
7. Analytical procedures	3.77 (4.51)	1.78 (3.36)	*0.063*

Panel C: Efficiency Scores by Procedure Category

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
1. Procedures to understand the marketing program and review contracts	12.20 (12.96)	7.02 (5.64)	*0.068*
2. Procedures to test revenue recognition	3.13 (4.63)	5.04 (5.00)	*0.098*
3. Confirmations	0.93 (1.99)	0.00 (0.00)	**0.035**

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TABLE 4 (continued)

Variable	Fraud Specialists (n = 32) Mean (Std. Dev.)	Auditors (n = 16) Mean (Std. Dev.)	p-value
4. Inventory-related procedures	2.68	1.39	0.117
	(3.90)	(2.53)	
5. Procedures to test allowance for bad debts	3.38	3.13	0.414
	(3.31)	(4.43)	
6. Cut-off tests	0.80	0.00	0.146
	(3.00)	(0.00)	
7. Analytical procedures	3.85	1.71	**0.051**
	(4.56)	(3.29)	

All p-values are based on a t-test and are one-tailed given the directional nature of H2b and H3. Bold values are equal to or below 0.05; values in italic are equal to or below 0.10.

Similarly to standard procedures, the effectiveness and efficiency of additional procedures are positively correlated.[5] Therefore, we perform a MANCOVA with additional procedure effectiveness and efficiency scores as dependent variables and *Participant Type* (fraud specialists versus auditors) as the independent variable, and audit experience and number of procedures proposed as covariates. *Participant Type* is marginally significant ($F(2,41) = 2.023$, $p = 0.072$, one-tailed, not tabulated), while both covariates are significant (for audit experience $F(2,41) = 3.304$, $p = 0.047$; for number of additional procedures proposed $F(2,41) = 525.567$, $p < 0.001$, not tabulated). These results suggest that fraud specialists' expertise makes a contribution to planning additional effective and efficient audit procedures beyond that which comes from their audit experience.

To test H2b further, we perform a series of ANCOVAs with effectiveness of additional procedures as a dependent variable, *Participant Type* as a fixed factor, and audit experience and number of additional procedures proposed as covariates. Using the same logic as in tests of H1b, we do not include efficiency as a covariate in these tests. We find no difference between fraud specialists and auditors ($F(1,42) = 0.002$, $p = 0.964$, two-tailed, not tabulated), although the number of additional procedures proposed is significant ($F(1,42) = 838.361$, $p < 0.001$, two-tailed, not tabulated). The marginal means for effectiveness of additional procedures score for fraud specialists and for auditors are 24.613 and 24.545, respectively. We interpret these results to indicate that additional procedures proposed by fraud specialists are not necessarily more effective by their nature than those proposed by auditors; they appear to be marginally more effective due to their greater number. They also appear more effective when analyzed in combination with their efficiency ratings. These findings indicate limited support for H2b.

Tests of H3

H3 states that fraud specialists' proposed additional procedures will be less efficient than those proposed by auditors. We use cumulative efficiency scores of additional procedures, calculated in the manner similar to effectiveness scores described in the previous section, as a dependent variable to test this hypothesis. In Table 4, Panel A, the overall efficiency of additional procedures proposed by fraud

[5] Correlations between effectiveness and efficiency ratings of additional procedures were 0.980 ($p < 0.001$, two-tailed) for the participant sample and 0.945 ($p < 0.001$, two-tailed) for the expert panel.

TABLE 5

Tests of H3
Analysis of Efficiency of Additional Procedures

Panel A: ANCOVA for Efficiency of Additional Procedures (*Efficiency of Additional Procedures*) as a Dependent Variable, by *Participant Type* (Fraud Specialists versus Auditors)

Source	df	F	p-value (two-tailed)
Participant Type	1	4.045	**0.051**
Audit Experience	1	5.058	**0.030**
Effectiveness of Additional Procedures	1	23.373	**0.000**
Number of Additional Procedures	1	11.103	**0.002**
Error	43		

p-values in bold are equal to or below 0.05.

Variable Definitions:
Audit Experience = number of years of audit experience, a covariate;
Number of Additional Procedures = number of non-redundant, meaningful additional procedures proposed by a participant as a supplement to the standard program for the audit of revenue cycle, a covariate;
Effectiveness of Additional Procedures = overall effectiveness score of additional procedures, a covariate; and
Participant Type = fraud specialists or auditors, a fixed factor.

Panel B: Marginal Means for Efficiency of Additional Procedures, by *Participant Type* (Fraud Specialists versus Auditors)

Participant Type	Marginal Mean	Standard Error
Fraud Specialists	23.712	0.719
Auditors	26.392	1.029
Grand Mean	24.052	0.586

Covariates appearing in the model in Panel A are evaluated at the following values:
Audit Experience = 8.65;
Effectiveness of Additional Procedures = 24.59; and
Number of Additional Procedures = 3.98.

specialists is 26.96 (Std. Dev. = 24.68) versus 18.28 (Std. Dev. = 10.12) for those proposed by auditors. A t-test of differences in mean efficiency scores of the additional procedures shows a marginally significant difference between fraud specialists and auditors (Table 4, Panel A: $p = 0.093$, one-tailed). This indicates a preliminary lack of support for H3 in that the overall efficiency of additional procedures is marginally greater for fraud specialists than for auditors, whereas H3 predicted that the overall efficiency of additional procedures would be lower for fraud specialists than for auditors.

To test H3 further, we performed a series of ANCOVAs with efficiency of additional procedures as a dependent variable, *Participant Type* as a fixed factor, and audit experience, number of additional procedures proposed, and effectiveness of the proposed procedures as covariates. We include effectiveness of additional procedures as a covariate based on the logic that an efficient procedure must be effective; otherwise, it is useless. Table 5 reports the results.[6] We find that efficiency of additional procedures proposed by fraud specialists is lower than the

[6] MANCOVA results are discussed in the "Tests of H2" section.

efficiency of auditors' additional procedures (Table 5, Panels A and B: $F(1,43) = 4.045$, $p = 0.026$, one-tailed, marginal means are 23.712 for fraud specialists and 26.392 for auditors). Thus, H3 is supported.

Tests of H4

H4 states that fraud specialists would revise the time budget for a standard audit program of the revenue cycle to a greater degree than auditors. Therefore, we compare revisions to the previous year's budgeted hours for the various categories of procedures by fraud specialists and auditors. Table 2, Panel B indicates that there is no significant difference between fraud specialists and auditors in terms of the proposed revision to the overall time budget for the audit program ($p = 0.356$, one-tailed, with mean revisions of 18.50 percent relative to the original budget for fraud specialists and 20.44 percent for auditors). However, fraud specialists revised the time budget differently from auditors for the following categories of procedures: tests of the aged trial balance (mean increase of 1 percent by specialists versus mean decrease of 5 percent by auditors, $p = 0.042$, one-tailed); cut-off tests (mean increase of 6.53 percent versus mean increase of 1.56 percent, respectively, $p = 0.002$, one-tailed); and for other tests (mean increase of 6.88 percent versus 23.12 percent, respectively, $p = 0.006$, one-tailed).

In comparison with Asare and Wright's (2004) auditors, who made no meaningful adjustments to the time budget, both auditors and fraud specialists in this study adjusted the total time budget significantly upward ($p < 0.001$ for mean increase to budgeted hours above zero). To recognize variations in the direction of adjustments (increase, decrease) to the time budget for individual groups of procedures, we use non-parametric tests. Wald-Wolfowitz runs, Mann-Whitney U Tests, and Kolmogorov-Smirnov tests indicate fraud specialists proposed statistically significantly different changes to the time budget than did auditors in the following areas of the standard program: test of aged trial balance, cut-off tests, and other tests (all $p < 0.01$, two-tailed), with a greater number of increases (based on ranks) to the budgets for the former two groups of procedures, and a lower number of increases to the budget for the latter.

Fraud specialists increased the time budgets for *all* types of procedures ($p < 0.001$), but they only did so to a significantly greater extent than auditors for tests of the aged trial balance and for cut-off tests. For *other procedures,* while proposing an increase in the time budget, fraud specialists actually suggested a smaller increase than auditors (Table 2, Panel B: 6.88 percent versus 23.12 percent, respectively, $p = 0.006$, one-tailed). Thus, H4 is not supported. Combined with the results of tests of H2a, which indicated that the fraud specialists added significantly more additional procedures than did auditors, this finding may also indicate that fraud specialists' time budgets may not accurately reflect the amount of time it takes to perform the greater number of procedures they recommended.

In summary, we observe that fraud specialists appear to increase the number of additional procedures without proportionate adjustments to the total time budgeted for those procedures. That is, they proposed a greater number of other additional procedures that are also more effective than those proposed by auditors (see discussion of H2a and H2b analyses), but they did not propose proportionate increases in the time budget for these tests (H4).

Additional Analyses: Overall Program Quality

To investigate our findings further, we analyze the overall quality of the audit programs proposed by fraud specialists and auditors. First, to supplement our analyses of H1b and H2b, we compare the overall effectiveness of all procedures, standard and extended, across fraud specialist and auditor groups. An ANCOVA using the cumulative effectiveness scores for standard and additional procedures as a dependent variable showed that *Participant Type* was not significant

($F(1,41) = 0.553$, $p = 0.461$, marginal means are 125.542 for fraud specialists and 124.043 for auditors, not tabulated), while the following control variables were statistically significant: number of procedures selected from the standard program ($F(1,41) = 4205.578$, $p < 0.001$) and number of additional procedures proposed by the participant ($F(1,41) = 404.369$, $p < 0.001$) (not tabulated). We interpret these results as indicating that while fraud specialists' proposed additional procedures are marginally more effective than those proposed by auditors (H2b), due to the lack of similar superior effectiveness in selecting standard procedures (H1b), the cumulative effectiveness of their audit programs is not higher than that of auditors. A limitation of this analysis is that it solely measures the cumulative effectiveness of the individual procedures, but does not assess synergies that may exist among procedures.

Second, we perform a similar analysis to supplement our tests of H3. We compare the overall efficiency of all procedures, standard and additional, between the two participant groups. ANCOVA results (not tabulated) indicate that *Participant Type* is not significant ($F(1,41) = 1.292$, $p = 0.131$, marginal means are 120.412 for fraud specialists and 121.967 for auditors), while the following covariates are significant: number of additional procedures proposed by the participant ($F(1,41) = 39.908$, $p < 0.001$), overall program effectiveness ($F(1,41) = 8271.151$, $p < 0.001$), and years of audit experience ($F(1, 41) = 3.657$, $p = 0.063$). Thus, it appears that when we consider overall program efficiency, fraud specialists' performance is not significantly different from auditors' (see, also, tests of H3).

Role of Audit Experience

Both the fraud specialists and the auditors who participated in this study had a significant amount of audit experience. Table 1 indicates that in our sample, fraud specialists had an average of about six years of audit experience and 12 years of specialized fraud experience, whereas auditors had an average of 13 years of audit experience, but no specialized fraud experience. The greater audit experience of the auditors may compensate, to a degree, for their lack of specialized fraud experience and may make it difficult to tease out the contribution of the fraud specialists to the audit program plan. Therefore, as we tested H1b, H2b, and H3, we performed analyses with audit experience as a covariate to determine whether fraud specialists' audit experience contributes to the effectiveness and efficiency of their audit program-related planning decisions. In these analyses, audit experience was significant only when we focused on the efficiency of additional procedures (Table 5, Panel A: $F(1,43) = 5.058$, $p = 0.030$).

V. CONCLUSIONS, DISCUSSION, AND LIMITATIONS

Asare and Wright (2004) found that their auditor participants were able to identify heightened fraud risks, but were unable to develop an effective audit program to respond to those fraud risks. They concluded that auditors' willingness to consult with risk management partners was a benefit because they would compensate for the auditors' program-planning limitations in the context of elevated fraud risk. Our study addresses this conjecture, specifically with respect to the involvement of fraud specialists. First, we find that in a setting with heightened fraud risk, the fraud specialists did not select a greater number of procedures from a standard audit program, and the procedures they selected were not more effective than those proposed by financial statement auditors. This suggests that the benefits of involving fraud specialists in audit planning do *not* lie in their ability to identify more effective *standard* audit procedures.

Second, we find that when the risk of fraud is other than low, specialists proposed more additional (non-standard) procedures that were also marginally more *effective* than the additional procedures proposed by auditors (H2a, H2b). On average, fraud specialists proposed about twice as many additional procedures as the auditors did, and those procedures were of a greater variety and,

in some cases, more effective than those proposed by auditors (Table 4, Panel B: $p < 0.05$ for categories 1 and 3, and $p \leq 0.10$ for categories 2, 4, and 7). This suggests that involving fraud specialists in audit planning can carry benefits for engagements where fraud risk is not low by helping to identify a larger set of effective procedures than even very experienced auditors are able to do.

Third, we find that when fraud risk is other than low, fraud specialists proposed additional procedures that were less *efficient,* as judged by audit experts, than the additional procedures proposed by auditors (H3). Also, although fraud specialists added almost twice as many additional (non-standard) procedures as the auditors did, they did not propose a proportionate increase in the audit budget (H4). Fraud specialists' budget increase for additional procedures was about one-third of the size of the budget increase proposed by the auditors (Table 2, Panel B: 6.88 percent versus 23.12 percent, $p = 0.006$). Fraud specialists may not consider increases in the time budget as much as auditors do because fraud-related work is more likely to be billed over and above initially agreed fee levels.

When we consider the results of our tests of hypotheses cumulatively, the following picture emerges. Although both auditors and fraud specialists added non-standard procedures to the audit program, fraud specialists seem to budget less realistically for the number of procedures added. Auditors actually cut the budgets for some standard procedures, making room in the overall audit budget for non-standard additional procedures. In contrast, fraud specialists added standard procedures, but they were not more effective than those selected by auditors, and also provided less budget room for those procedures. Further, specialist-proposed additional procedures were marginally more effective, but less efficient, than those that were added by financial statement auditors.

We conclude that the involvement of fraud specialists in planning an audit engagement where fraud risk is present is likely to lead to additional audit effort and cost, possibly without commensurate benefit. However, considering the potential consequences to the auditor of undiscovered fraud, it may be cost-effective to include additional non-standard procedures in an audit program if they improve the probability of discovering a fraud. This is particularly important since one area in our study where fraud specialists' recommended procedures were significantly more effective than auditors' involved procedures aimed at understanding the marketing program and reviewing contracts, which were key problem areas in the case that we used. Engagement partners will have to weigh the risk-reducing modifications and extensions of the audit programs proposed by fraud specialists against the budget constraints of the audit engagement.

We have the following caveats for our research. First, we relied on individual judgments made by auditors and specialists, as opposed to audit team decision-making. Second, we did not incorporate a fraud brainstorming session in our experiment. Third, we relied on audit experts rather than fraud experts for independent judgments of the effectiveness and efficiency of participant-proposed procedures, following the reasoning that an audit engagement partner makes the final decision regarding which specialist-proposed procedures to include in the audit plan. Also, the expert panels were not told that the case was based on a fraud case, so their effectiveness and efficiency ratings relate to the situation where an elevated, but not extreme, risk of fraud presented itself. Those ratings might have differed had they known that an actual fraud had been discovered in the case company. However, providing the expert panels with knowledge that a fraud had occurred would have provided them with hindsight knowledge that might have biased their assessments of the procedures by converting the uncertainty about the fraud risk into a certainty. This could have changed the task in their mind from an audit-planning task into a fraud investigation task. A potential extension of this study would be to have a panel of fraud experts assess the procedures after being advised that a fraud had occurred. Fourth, all of our participants were from Canada. Fifth, both the auditors and the fraud specialists had high levels of prior audit experience.

Differences between the audit programs designed by less experienced auditors and fraud specialists could be more pronounced. Finally, our experimental materials were limited in terms of amount and quality of information compared to what auditors and fraud specialists would typically have access to in the field.

With these caveats in mind, this research contributes to a deeper understanding of the usefulness of seeking the assistance of a fraud specialist to help determine the best strategy for auditors to use when they encounter risk factors that signal a higher than low risk of fraud. Future extensions of this research could focus on how the fraud specialists' precise role affects their contribution to the audit. For example, does the quality of the contribution change if the fraud specialist plays a more extensive role in the audit team, with several points of interaction with the auditors during an engagement, as compared to a role that is limited to responding to the auditors' request for assistance only when certain conditions are met? What are the benefits of involving fraud specialists at various stages of an audit engagement? In this regard, Martin, Rich, and Wilks (2006) call for research into how the current structure of audit teams affects firm utilization of specialist knowledge and whether the incentive and reward structure needs to change as the number of necessary specialists used in an audit (e.g., tax, valuation, fraud, and IT specialists) increases. Another avenue of interest is how engagement partners deal with efficiency and budget issues when considering the use of specialists. For example, given our findings, engagement partners might consider whether it is more beneficial to involve very experienced auditors rather than fraud specialists in the audit-planning phase of engagements where fraud risk is elevated.

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APPENDIX A

Ratings of Standard Procedures by the Expert Panel, and Proportions of Participants Who Selected Individual Procedures from the Standard Audit Program for the Revenue Cycle

Standard Audit Program for Revenue Cycle from Asare and Wright (2004)	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists (n = 32) %[a]	Auditors (n = 16) %
Aged Trial Balance				
Obtain an aged trial balance of trade receivables as of the date selected for confirmation procedures. Perform the following:				
a. Cross-foot the totals and re-foot the total column and analysis columns.	5.6	6.2	59%	56%
b. Trace total to the general ledger control account and to the lead schedule or working trial balance.	6.9	7.2	78%	75%
c. On a test basis, trace entries for individual customers on the aging analysis (totals and aging detail) to the individual accounts in the accounts receivable subsidiary ledger, and select individual accounts from the subsidiary ledger and trace totals and aging detail to the aged trial balance to determine if aging is correct. Test footings of individual customer accounts in the subsidiary ledger.	6.2	5.3	69%	50%
Confirmations				
Select individual customer accounts for confirmation procedures from the aged trial balance and arrange for the preparation of confirmation requests to be mailed under the auditor's control and tested as follows:				
a. Trace individual confirmation requests as to balances and addresses to the subsidiary accounts receivable records.	8.1	7.8	63%	69%
b. Send confirmations (using envelopes with the auditor's return address) and prepare confirmation statistics.	9.3	8.2	81%	83%
c. Trace confirmation replies to the trial balance and investigate replies with differences.	8.0	6.7	78%	83%
d. Obtain new addresses for all confirmations returned by the post office and remail.	7.8	6.6	66%	75%
e. Send second requests for all unanswered positive confirmation requests. Consider sending third requests by registered or certified mail and performing alternative auditing procedures.	6.0	5.4	72%	69%

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Standard Audit Program for Revenue Cycle from Asare and Wright (2004)	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists (n = 32) %[a]	Auditors (n = 16) %
f. Ascertain whether any accounts or notes have been assigned, pledged, or discounted by reference to minutes, review of agreements, confirmation with banks, etc.	6.9	6.5	69%	83%
Adequacy of Bad Debt Allowance				
Obtain or prepare an analysis of the allowance for doubtful accounts for the period and review adequacy of the allowance and related provision by:				
a. Review the aged trial balance as of the balance sheet date with the client's credit manager or other responsible individual to identify accounts of a doubtful nature and allowances required; review correspondence files and other relevant data in support of client's representations. Items reviewed should include past-due amounts and significant amounts if past due.	7.7	7.4	72%	83%
b. Examine credit reports for delinquent and large accounts.	7.3	6.4	72%	63%
c. Review confirmation exceptions for indication of amounts in dispute.	7.9	7.3	72%	63%
d. Consider requesting audited financial statements for large accounts that are past due and appear doubtful.	6.0	3.3	34%	63%
Cut-off Tests				
Perform cut-off tests for sales and returns				
a. Select sales invoices for testing from the sales register for several days before and after year-end and examine shipping records and determine that they were recorded in the proper period.	7.4	6.6	72%	83%
b. Select credit memos issued after year-end and examine underlying documentation (for example, record of receipt of returned goods) to determine period to which credit memo is applicable and whether it was recorded in the proper period.	7.9	7.3	75%	83%
Analytical Procedures				
Analyze and review trends for the following relationships:				
a. Accounts receivable to credit sales.	7.3	7.9	75%	63%

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APPENDIX A (continued)

Standard Audit Program for Revenue Cycle from Asare and Wright (2004)	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists (n = 32) %[a]	Auditors (n = 16) %
b. Allowance for doubtful accounts to accounts receivable (in total and in relation to past-due categories per aging analysis).	7.9	7.8	75%	81%
c. Sales to returns and allowances.	6.5	6.7	75%	75%
d. Expense provisions for doubtful accounts to net credit sales.	6.2	6.9	63%	50%
e. Expense provisions for doubtful accounts to write-offs.	5.7	6.6	53%	50%
f. Moving average relationship of write-offs to trade receivables.	4.9	6.4	50%	13%
g. Average balance per customer.	2.9	3.7	44%	19%
h. Ratio of accounts receivable to current assets.	3.0	4.2	47%	6%

[a] % = percent of participants who responded that a standard procedure should be used in the audit program for revenue cycle.

APPENDIX B

Additional Procedures Proposed by Auditors and Fraud Specialists

Panel A: List of Additional Procedures Proposed by Auditors and Fraud Specialists

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
1.0 Understand the Program/Review Contracts					
1.1 If available, read minutes of the November 13 meeting with distributors.	A&W1	5.3	5.9	—	—
1.2 If minutes (of November 13 meeting with distributors) not available, inquire of a sample of distributors at the meeting to ascertain their understanding of issues discussed.	A&W5	2.9	1.4	—	—
1.3 Review the documentation prepared by Precision to estimate the effects of the new marketing program. Identify costs factored into the analysis of the program and what costs have been omitted (if any). Examine and recalculate company's estimate of costs and revenue contra amounts and those actually accrued (if any).		7.3	7.1	1 (3.1%)	2 (12.5%)

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
1.4 Inquire from Internal Auditor and Audit Committee as to their level of understanding of the accounting implications of the marketing strategy. Review whistleblower channel reports for evidence of override activity.		5.4	5.4	2 (6.3%)	—
1.5 Discuss the accounting memo and results of audit procedures with the President to ensure he understands the issues and makes informed management representations.		4.0	4.3	1 (3.1%)	—
1.6 Inquire of distributors who committed on November 13 as to their understanding of the terms of the sales.	A&W2	3.0	2.2	2 (6.3%)	1 (6.3%)
1.7 Inquire of distributors who were at the November 13 meeting and who did not commit to participate in the marketing program as to their reasons for not committing to the program.	A&W3	1.5	1.1	2 (6.3%)	—
1.8 Inquire of undecided distributors who changed their minds between November 13 and year-end, the reasons for changing their minds.	A&W4	2.3	1.5	1 (3.1%)	—
1.9 Inquire of distributors whether there are any: return privileges for the customer; extended payment terms; side agreements to the sale terms; rights to return; financing provided to distributors by Precision.		6.5	5.7	4 (12.5%)	—
1.10 Inquire of finances, sales, and accounting personnel, and credit and collections department whether they are aware of any situation where revenue has been improperly recognized.		5.1	6.0	1 (3.1%)	—
1.11 Inquire of individuals outside of the accounting department who initiate or process transactions with distributors/customers about undisclosed side agreement by management or activities that are out of the ordinary course of business.		6.3	7.2	2 (6.3%)	2 (12.5%)
1.12 Inquire of sales, shipping personnel, and credit and collections department whether they are aware of any significant sales or shipments to unfamiliar new customers.		4.3	4.9	2 (6.3%)	—

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
1.13 Confirm with end-users that Precision was encouraging them to buy from the distributors.	A&W10	1.6	1.0	1 (3.1%)	—
1.14 Review correspondence file with distributors for evidence of side agreements (e.g., right of return, payment terms).	A&W6	4.7	4.8	1 (3.1%)	—
1.15 Review the terms and conditions under which Precision will accept returns of equipment from distributors.		8.7	8.9	6 (18.8%)	1 (6.3%)
1.16 Review Promissory Notes for any return terms for unsold product.		8.5	8.5	2 (6.3%)	—
1.17 Conduct a keyword search of company email to determine if any special arrangements exist as to funding or if any other unusual transactions exist without auditors' knowledge.		2.9	1.5	1 (3.1%)	—
1.18 Review new contracts and agreements. Compare with drafts of these agreements to ensure that negotiations did not result in changes that include round-trip schemes.		6.2	5.7	1 (3.1%)	—
1.19 Obtain copies of the agreements with the distributors to see if there are clauses that could affect the level of sales (when the transfer of property occurred, the return policy, volume discounts after year-end, hidden commission).		8.5	8.2	3 (9.4%)	—
1.20 Determine the impact of the marketing program on management compensation by comparing this year's bonus to top management (by employee) to prior year's to see how much individuals are benefiting from the marketing program.		6.0	6.1	3 (9.4%)	1 (6.3%)
1.21 Check disclosure/calculation of contingent liabilities relating to newly introduced points programs.		8.6	8.3	8 (21%)	5 (31.3%)
1.22 Review the profit-sharing option to ensure accuracy of profit-sharing calculations and make sure they are recorded in the financial statements as a potential cost per EIC 156 and that expenses are appropriately matched to revenues.		8.4	8.6	9 (28.1%)	2 (12.5%)

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
1.23 Ascertain whether there are any rebates given on sales to customers. Rebates should be offset against revenues.		7.9	7.9	2 (6.3%)	—
1.24 Obtain an understanding and support for both the redemption rates and cost per point used in determining the cost/liability of the new loyalty program.		8.3	7.8	—	1 (6.3%)
1.25 Review the debt covenant for the increased debt in 2005.		4.5	4.6	1 (3.1%)	—
2.0 Revenue Recognition					
2.1 Review revenue recognition policies for consistency to prior year.		7.7	8.5	1 (3.1%)	2 (12.5%)
2.2 Review sales contracts for November marketing program to assess whether revenue recognition is appropriate in accordance with EIC 141; Need to consider impact on revenue recognition of: extended credit terms, rights of return/consignment, and warehousing of goods.		8.7	8.4	4 (12.5%)	5 (31.3%)
2.3 Perform the following procedures on the sample of sales to distributors: Obtain the contract; Ascertain whether the four revenue recognition criteria are met for each of the contracts; Determine whether Precision assisted the customer in obtaining financing or provided direct financing for the sales.		6.6	6.6	1 (3.1%)	—
2.4 Inquire of management regarding any bill and hold arrangements. If so, examine sales contracts for evidence if a sale actually occurred and if all risks with rewards have been transferred.		6.8	7.6	2 (6.3%)	2 (12.5%)
2.5 Inquire if there are any misstatements in the amount of merchandise the company ships or receives or if there has been destruction, concealment, predating or post-dating of shipping and/or inventory documents.		4.1	5.9	1 (3.1%)	—
2.6 Get listing of all inventories picked, but not yet delivered to customers, held at warehouse or under accommodations. Examine evidence of delivery where revenue has been recognized.		7.6	6.7	2 (6.3%)	—

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
2.7 Perform a system query to obtain a listing of customers who received loans from Precision. Investigate any distributions and sales that are comparable in timing or amount. Obtain supporting documentation to understand the nature of the distribution, as well as whether all revenue recognition criteria have been met.		4.5	4.0	1 (3.1%)	—
2.8 Review terms under which Precision ships goods to customers to ensure they are based on a purchase contract and that no channel stuffing has occurred.		6.6	6.4	2 (6.3%)	1 (6.3%)
3.0 Confirmations					
3.1 Select sample of distributors with large balances for confirmation of inventory on hand and amount receivable by Precision. Contact any non-replies by phone directly to inquire of status of account. (In the circumstances, shipment records or subsequent payments may be inappropriate and of questionable value.); Make enquiry of four distributors regarding their allotment minima; Request and review schedule of allotments of product to distributors that must have been prepared by the Company.		6.5	5.7	3 (9.4%)	—
3.2 Make a selection of sales transactions throughout the period to significant customers. For each selection, prepare and send a confirmation of terms of sales, and request confirmation that no other side agreements or terms govern such transactions—specifically, inquire re: existence of right of return. Examine original docs to support transactions, including "ship to" info—determine if the product was going to customer or warehouse. Determine if there is valid PO and that procedures to validate customer have been completed (e.g. credit report, contact info, etc.).		4.7	4.2	3 (9.4%)	—

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APPENDIX B (continued)

Procedures by Category	**Cross Reference to A&W**	**Average Effectiveness Rating**	**Average Efficiency Rating**	**Fraud Specialists n (%)**	**Auditors n (%)**
4.0 Inventory/Warehouse					
4.1 Investigate the rationale for Precision's involvement in the storage and warehousing of distributor sales.	A&W7	4.6	5.2	—	I (6.3%)
4.2 Confirm with distributors all amounts removed from Precision's inventories, but not yet delivered to distributors, and held at warehouse or under accommodations, to ensure they meet the requirements for revenue recognition according to GAAP. Tie these amounts into sales contracts.		7.5	6.6	1 (3.1%)	—
4.3 Assess ownership of goods in offsite storage: Has legal title passed? Who owns offsite storage? Who is responsible for rent? Who pays insurance?	A&W8	7.0	7.2	4 (12.5%)	1 (6.3%)
4.4 Review shipments to distributors to make sure items shipped to distributors that were recorded as sales were not at the same time also in inventory in rented warehouse facilities.		4.6	4.6	3 (9.4%)	—
4.5 Review shipment of goods to third-party warehousing facilities and ensure revenue is not recognized.		4.5	4.9	4 (12.5%)	2 (12.5%)
4.6 Determine whether inappropriate adjustments to inventory have been made and Precision is not inflating sales/understating obsolete inventory (e.g. analog versus digital technology, etc.).		5.8	5.7	3 (9.4%)	—
5.0 Allowance for Bad Debts					
5.1 Compare orders taken via the marketing program to authorized credit limit.	A&W13	3.6	3.8	2 (6.3%)	—
5.2 For all increases in credit limits, review client analysis of distributor creditworthiness. If no analysis exists, perform probing, substantive analysis of distributors' creditworthiness.	A&W9	3.5	3.6	7 (21.9%)	3 (18.8%)

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
5.3 Obtain list of distributors with increased limits; send A/R confirmation to customers outstanding at December 2005; review aging of A/R for these customers to assess if balances are greater than 90 days. Determine if an additional provision should be estimated, or should the sales be reversed.		5.3	5.1	5 (15.6%)	—
5.4 Look at subsequent cash receipts for some of the large distributor sales.	A&W12	5.4	5.5	—	1 (6.3%)
5.5 To determine if distributors are on track for selling 30 percent (based on Precision's estimate) of their inventory by June 2008, when their promissory notes are due to Precision, obtain analog sales (# of items) data for November and December 2007 from the 11 distributors participating in the marketing program. Compare distributors' sales to third parties to their analog purchases from Precision to ensure sales targets will be met.		4.8	4.6	1 (3.1%)	—
5.6 Interview management, finance, accounting, and sales personnel to identify sales transactions with customers deemed to be un-creditworthy.		3.6	4.4	1 (3.1%)	—
5.7 Obtain download of customer list. Run against credit rating agency database. Focus in particular on sales near period end. Examine supporting documentation to ascertain whether collectability at point of revenue recognition was reasonably assured.		3.9	2.4	3 (9.4%)	—
5.8 Test sample of promissory notes to ensure that they are executed by both parties.		5.2	5.7	1 (3.1%)	—
5.9 Request audited financial statements for distributors with promissory notes to assess their credit risk.		3.5	2.6	—	1 (6.3%)
5.10 Review promissory notes from each participating distributor in the promotion program. Determine if promissory notes are appropriately secured.		5.6	4.6	1 (3.1%)	2 (12.5%)

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
5.11 Send confirmations of all promissory notes outstanding.		7.3	7.3	2 (6.3%)	3 (18.8%)
5.12 Determine whether sales prices of marketing program goods are reasonable, on par with what the marketplace wants and not indicative of obsolete goods (i.e. overpriced).		3.8	4.0	1 (3.1%)	—
5.13 Make a selection of credits booked to Accounts Receivable during period subsequent to December 2005. Examine supporting documentation to determine if sales transactions are properly recorded.		5.4	4.6	1 (3.1%)	—
6.0 Cut-Off Tests					
6.1 Extend sales cut-off procedures from five days to 15 days and extend credit note review to date of completion of field work.		4.6	3.5	1 (3.1%)	—
6.2 Review any entries posted to the contra revenue account prior to or near the period end.		5.1	5.1	1 (3.1%)	—
6.3 Make a selection of revenue posted just a few days before the end of the reporting period. Confirm with the customers that the orders were valid.		4.7	3.9	1 (3.1%)	—
6.4 Make a selection of revenue posted just a few days before the end of the reporting period. Determine if the selected sales meet GAAP requirements for revenue recognition and, in this case, customer acceptance of goods (transmission of ownership rights).		7.1	6.6	2 (6.3%)	—
7.0 Analytical Procedures					
7.1 Analyze monthly analog sales and order trends for company and industry. Assess impact of program on historical purchases by each of the known customers. If sales through marketing program would be increasing analog product in the marketplace to an implausible level (market saturation), assess the consequences to Precision of the distributors still having significant quantities on hand.		6.0	5.5	2 (6.3%)	4 (25%)

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APPENDIX B (continued)

Procedures by Category	Cross Reference to A&W	Average Effectiveness Rating	Average Efficiency Rating	Fraud Specialists n (%)	Auditors n (%)
7.2 Review and analyze product shipped under the marketing program to identify and explain large or unusual changes in pricing or product mix.		5.7	5.5	3 (9.4%)	—
7.3 Compare Q3 budget to actual. Compare revenues and expenses to budget/forecast and identify any large difference, obtain explanation, and ensure to corroborate with other evidence.		4.6	5.4	2 (6.3%)	1 (6.3%)
7.4 Compare the sales in the first quarter to that indicated by Precision's operating plan.	A&W11	3.9	4.2	—	—
7.5 Review fourth-quarter operating results by product group—analog/digital. Compare to the disclosed results of program during its eight weeks of operation. Compare to the trends of the first three quarters. Review and understand impact of working capital adjustments in cash flow.		5.2	5.3	4 (12.5%)	—
7.6 Analyze and assess the historic rates of returns for participation in marketing programs and actual returns during field work period to determine if provision needed and, if so, the adequacy of management's provision.		5.6	5.6	8 (25%)	—
7.7 Obtain profit estimates/forecasts/analyst expectations—calculate impact of sales resulting from "incentive program" on results and compare to expectations and incentive compensation plan.		5.4	6.1	4 (12.5%)	—

A&W numbers are the identification numbers of the benchmark procedures in Asare and Wright's (2004, Exhibit 3).
Effectiveness and Efficiency Average Ratings were obtained from ten audit partners/managers in public practice; the scales were anchored as follows: 0 = not at all effective/efficient, to 10 = very effective/very efficient.
Chi-square tests for differences in proportions by category of procedures between fraud specialists and auditors:
Category 1.0 "Understand the Program/Review Contracts": $\chi^2(6, n = 48) = 8.639$, $p = 0.098$;
Category 2.0 "Revenue Recognition": $\chi^2(2, n = 48) = 4.267$, $p = 0.059$;
Category 3.0 "Confirmations": $\chi^2(1, n = 48) = 6.621$, $p = 0.005$;
Category 4.0 "Inventory/Warehouse": $\chi^2(2, n = 48) = 3.422$, $p = 0.091$;
Category 5.0 "Allowance for Bad Debts": $\chi^2(3, n = 48) = 8.347$, $p = 0.020$;
Category 6.0 "Cut-off Tests": $\chi^2(2, n = 48) = 3.148$, $p = 0.104$; and
Category 7.0 "Analytical Procedures": $\chi^2(3, n = 48) = 4.737$, $p = 0.096$.

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APPENDIX B (continued)

Panel B: Frequency of Additional Procedures Proposed by Fraud Specialists and Auditors

Number of Additional Procedures Proposed	Fraud Specialists n = 32		Auditors n = 16	
	n	%	n	%
0	3	9.4	1	6.3
1	3	9.4	3	18.8
2	4	12.5	2	12.5
3	4	12.5	4	25.0
4	6	18.8	5	31.3
5	4	12.5	1	6.3
6	4	12.5	—	—
8	1	3.1	—	—
12	1	3.1	—	—
14	1	3.1	—	—
18	1	3.1	—	—
Total	32	100.0	16	100.0

Chi-square test for differences in proportions for all additional procedures between fraud specialists and auditors: $\chi^2(10, n = 48) = 13.885$, $p = 0.027$, one-tailed.

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Optimal Information Asymmetry, Control Environment, and Investment in Firm-Specific Human Capital

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ABSTRACT: When future operations are expected to provide information rents, managers concerned with being replaced can entrench themselves with value-increasing firm-specific human capital (SHC). In motivating SHC investment, the firm trades off the incentive effects of an *ex ante* commitment to asymmetric information against the costs of compensation rents and private benefits.

Firm value, therefore, is affected by (1) the accuracy with which the board observes and interprets information, and (2) the strength of the control environment restricting the manager's ability to benefit from concealing and diverting firm value. It is optimal to maintain a partially informed board to the mutual benefit of shareholders and managers, and for firms in a stricter control environment to maintain a more informed board. Due to the indirect effect on SHC, regulations that strengthen control adversely affect firm value unless the information and control environments are sufficiently biased toward managerial preferences.

Keywords: *human capital; information asymmetry; incentive contracting; hold-up problem; control environment; regulation.*

JEL Classifications: *G30; G38; M12; M48.*

The most valuable of all capital is that invested in human beings.
—Alfred Marshall (1959)

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I. INTRODUCTION

Given the strategic importance of human capital and the potential for employee mobility, a key challenge for firms is how to induce employees and managers to invest in firm-specific human capital (SHC). When such investment is unverifiable, and so cannot be compensated directly via incentive contracts, the manager may invest too little in SHC *ex ante* (Becker 1962).

We address this well-known "hold-up" problem in the context of managerial information advantages and the opportunity to conceal and divert hidden value for private benefit. In the presence of dynamically consistent optimal compensation contracts, we ask how seemingly unrelated information asymmetry interacts with the strength of the control environment and the threat of replacement to provide an incentive to develop value-increasing SHC. Specifically, SHC serves to "entrench" the manager by making him sufficiently valuable that the firm will not want to replace him, despite costly asymmetric information. In contrast to some other forms of entrenchment, we show that the entrenchment attributable to SHC is beneficial to managers *and* shareholders.[1]

Our question is important because firms and their regulators expend significant resources to ensure that relevant information is transmitted to the board and shareholders, and to safeguard the assets of the firm against extraction of private benefits by insiders. Recent legislation, such as the Sarbanes-Oxley Act ("SOX") and the Dodd-Frank Act, exhibits the natural reflex to treat information asymmetry as a "bad thing" that is ideally minimized through maximum transparency. Similarly, the legislation emphasizes a control environment that makes conversion of shareholder value to private benefits difficult to achieve and likely to meet with significant risks and consequences of detection. These priorities are understandable, as they can be socially optimal in a number of well-studied contracting settings where hidden actions and hidden knowledge combine to benefit managers at the owners' expense (e.g., Holmström 1979; Kofman and Lawarree 1993; Demski 1997; Harris and Raviv 1996; Rajan and Saouma 2006). However, we show that regulation may have unintended consequences on incentives to invest in SHC, whereupon practitioners risk wasting resources and failing to maximize shareholder value. More broadly, a proper appreciation of the nature of SHC is important to avoid suboptimal hiring and compensation decisions.[2]

Our first contribution is to illustrate how information asymmetry regarding exogenous profitability and the concealment and extraction of private benefits combine with the threat of firing to create an incentive for synergistic SHC investment. Information asymmetry is costly to shareholders, in part because it enables the manager *ex post* to earn strictly more than his reservation utility. However, this can be a "good thing" when it gives the manager an incentive to develop observable, but unverifiable, SHC *ex ante*. This occurs not because SHC enhances the manager's potential information rent next period, but because the relationship-specific value created deters the firm from replacing the manager, despite the cost of rents and inefficient private benefit extraction. In equilibrium, the manager chooses to invest in SHC, even while incurring the full cost

[1] Long-term contracts can help guarantee future returns to SHC. However, long-term guarantees of employment are themselves often costly when, for example, they constrain the option to wait for the resolution of uncertainty that renders contracting more efficient. We focus on the incentive to develop SHC even with dynamically consistent *spot* contracts.

[2] Cazier and McInnis (2010) document that external CEO hires come from firms that previously enjoyed stock price success and they receive a pay premium relative to internal hires. However, this pay premium is negatively related to subsequent operating performance at the hiring firm. This is consistent with an interpretation that outsiders' prior success was due to firm-specific abilities at their old firm, not easily applicable at the new employer. Even though Custódio, Ferreira, and Matos (2013) find evidence of increased importance (or at least prevalence) of general human capital in recent years, two-thirds of Forbes 1000 CEO appointments are still filled by insiders (*Economist* 2011), again suggesting a key role for accumulated SHC.

of doing so and having no bargaining power over the value created, i.e., despite the ingredients of the classic hold-up problem illustrated by Becker (1962).

Building on this distinctive interaction between SHC and information asymmetry, we examine how firm value is affected by two key features of the firm's information and control environment. The first is the precision with which the board observes and interprets information available to the manager, and we label this "board informedness." The second is the strictness of the controls on the manager's ability to divert hidden value. Board informedness is an *ex ante* choice variable in our analysis, reflecting initial decisions such as board composition, board expertise, and quality of the infrastructure created to collect and report appropriate hard and soft information to the board for the second-period contracting stage. Increasing board informedness has two effects on the contracting stage. First, the board is less likely to face a situation where the manager is better informed. Second, in the event that it does face a better-informed manager, the board is able to deal with the asymmetry at lower cost.

We impose no direct cost of increasing board informedness, so the firm could easily choose to eliminate information asymmetry if desired. However, this would leave the manager with no incentive to develop SHC, as entrenchment would provide him no benefit. With an imperfect information structure, there are some realizations of the board's signal where the manager is better informed and can expect to extract information rents. To ensure that he will be retained in those circumstances, the manager develops sufficient SHC *ex ante* to cover the firm's information costs that remain, even in the presence of an optimal (i.e., "cost-minimizing") second-period compensation contract. For other realizations, where no information asymmetry arises *ex post*, the firm extracts the value of the SHC at a profit because it incurs no contracting costs. Overall, the manager is better off due to the possibility of rents, and the firm is better off due to the profit created by SHC. The value-maximizing level of board informedness occurs where the reduction in the likelihood of *incurring* information costs offsets the reduction in SHC investment motivated by the magnitude of the *potential* information costs. This interior "win-win" outcome contributes a new dimension to the theoretical literature on optimal information asymmetry (Antle and Fellingham 1995; Rajan and Saouma 2006).[3] Empirically, this interaction should be most apparent in firms where SHC is important and the manager's position admits the possibility of costly information advantages.

A further contribution is to show that the optimal level of board informedness is positively related to the firm's control environment, which we define to capture the stewardship, regulation, investigation, and enforcement mechanisms that present impediments to conversion of hidden value to private benefit. The strictness of the control environment determines the extent to which the manager could expect to gain from exploiting his information advantage. It does this by affecting how difficult diversion would be to perform, how effectively diverted value could be transformed into private benefit, and the likelihood and consequence of detection.[4]

We introduce a parsimonious variable that captures these impediments. It affects the cost to the firm of the manager's information advantage and, hence, the offsetting SHC investment needed for the manager to retain his position. We find that stricter control reduces information costs and the rate at which they further decline with board informedness. Hence, stricter control reduces the rate

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[3] Lau (2008) also derives an interior optimum, but where the information asymmetry regards the relationship-specific investment itself. Her optimum relies on mixed-strategy equilibria associated with the unobservability of relationship-specific investment. In contrast, our analysis remains in the tradition of the classic hold-up problem with observable SHC (Grossman and Hart 1986; Hart and Moore 1988) and pure strategies.

[4] We acknowledge that the term "control environment" can have alternative connotations. In this article, it refers only to the mechanisms that affect the manager's incentive to expropriate firm value. Our initial analysis treats these as exogenously determined by regulations, laws, norms, etc. Our extended analysis ultimately admits the possibility of an endogenous control environment.

at which SHC declines with board informedness, so the firm optimally chooses a more informative structure, making control strength and board informedness complements. Therefore, a notable empirical prediction of our analysis is that a mandatory strengthening of the control environment will *indirectly* cause the firm to respond with measures to increase board informedness. Information asymmetry is related to the firm's organizational structure, as represented by Dutta and Fan (2012), who *define* centralization as the likelihood of the principal being informed. Given this interpretation, our endogenous complementarity result is consistent with Vakkur, McAfee, and Kipperman (2009), who find that managers perceive SOX to have produced unanticipated effects within their firms; most notably, centralization.

Finally, we extend our setting to allow for the possibility that the board's objective deviates from shareholder wealth maximization such that board informedness, and even the control environment, is influenced by the manager's personal preferences. This enables us to contribute to the debate over the ideal control environment. SOX, for example, requires management and the external auditor to attest to the adequacy of the company's internal controls, and introduces potential criminal liability in this regard.

When shareholder wealth maximization remains the board's objective, we find that a mandatory strengthening of control *reduces* firm value due to a subtlety of the hold-up problem. Similar to board informedness, stronger control reduces contracting costs in the case where information asymmetry arises, in turn reducing SHC *ex ante*. However, in contrast to board informedness, stricter control does not decrease the *likelihood* of facing contracting costs and so is dominated by board informedness as a mechanism to limit contracting costs when shareholder wealth is sufficiently prioritized.

The effect of a mandatory strengthening of control is *reversed,* however, if the design of the information environment is significantly biased toward the manager's personal preferences. Such a bias causes the firm to reduce board informedness below the value-maximizing level, permitting additional managerial rents. Stronger control now *increases* shareholder wealth, thanks to the complementary effect on the board informedness choice above—with sufficient weight on the manager's preferences, firms respond to a regulatory strengthening of their control environment in a manner that increases shareholder value.

Thus, our model predicts that a mandatory strengthening of control has an ambiguous effect on shareholder value that depends on board objectives and subtle links to SHC, suggesting a reason for caution from the regulator. Indeed, the empirical literature on SOX has produced contradictory conclusions on its *overall* effect on the value of public equity (e.g., Jain and Rezaee 2006; Zhang 2007), and our results may help explain this lack of consensus. In particular, our results suggest that empiricists should control for board objectives and the importance of SHC when testing for effects of control environment regulation. In this spirit, Hochberg, Sapienza, and Vissing-Jorgensen (2009) find that the firms whose insiders lobbied hardest *against* the implementation of SOX were those with the strongest evidence of agency problems and entrenched management. Those firms also enjoyed the greatest cumulative stock returns in the months leading up to the *passage* of SOX, suggesting that their shareholders had the most to gain from a stricter control environment, consistent with our model's prediction.

The corporate information environment has received considerable attention in the extant theoretical literature. Arya, Fellingham, Glover, and Sivaramakrishnan (2000) examine the relationship between information asymmetry, capital budgeting, and *ex ante* project search effort. They show that a coarse information system—an intermediate level of information asymmetry—can create budgetary slack, consumed without impediment as managerial perquisites (rents). In their model, manager effort affects uncertainty and so can directly increase the magnitude of rents obtained from greater budgetary slack. In contrast, our board informedness relates to an exogenous source of uncertainty that *cannot* be influenced by the manager's *ex ante* SHC choice. Abstracting

from any such straightforward link enables us to uncover an indirect "job preservation" motive for SHC investment. SHC secures the manager's *access* to the second period, where asymmetric knowledge of the exogenous profitability parameter guarantees information rents in the presence of an optimal compensation contract and other constraints on the consumption of hidden value as perquisites.

Dutta and Fan (2012) focus on the information structure associated with organizational form. They find that unobservable *ex ante* innovation effort can directly increase information rents in a manner that mitigates hold-up, suggesting a stark "fully centralize or fully decentralize" corner optimum for information asymmetry.[5] In contrast, our hold-up problem is mitigated by the *observable* value created by SHC, which serves to protect a potential information rent of magnitude that is unrelated to SHC.[6] This, together with the interplay between the likelihood *and* degree of information asymmetry, enables us to show how an interior, partially centralized structure could be optimal. Our analysis further suggests that the optimal degree of centralization is positively related to the strength of the firm's control environment.

Laux and Mittendorf (2011) focus on board independence, another factor affecting entrenchment. They illustrate a potential shareholder benefit of a board whose *ex post* objective is biased toward the manager's welfare. This leads to greater incentive pay, which deters pet projects *ex post* and, in turn, increases the incentive for initial effort. Our analysis illustrates that an *ex ante* bias toward the manager's welfare can also increase SHC, but in a manner that is unambiguously costly to shareholders. The extent of this bias, however, is pivotal in determining whether an exogenous strengthening of the control environment would benefit shareholders.[7]

In sum, the information and control environment that we attempt to capture is both important and complex. The above articles are part of an established literature employing various *ex ante* commitment devices to mitigate an *ex post* hold-up problem. This study's contribution is distinguished along a number of dimensions. First, we incorporate *independent* measures of informedness and control, each with their own novel features. Our informedness measure encompasses both the likelihood that the manager gains an information advantage and the degree of the advantage when it occurs. This creates a trade-off between the likelihood of incurring contracting costs versus the magnitude of SHC investment motivated, enabling us to derive a unique interior optimal board informedness. When the manager *does* obtain an information advantage, our measure of the control environment incorporates the factors that restrict his ability to benefit from the combination of value concealment and diversion, in a manner that distinguishes between benefits allowed and benefits deterred. Disentangling informedness and control in this way

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[5] Dutta and Fan (2012) model a discrete "delegate or centralize" choice, where the dilemma is between certainly having an uninformed principal or having the principal potentially fully informed (with exogenous probability). Whether delegate or centralize dominates is independent of the exogenous probability of being informed under centralization. The firm's profitability is monotonic in the board's probability of being informed, implying that their optimal asymmetric information would always be a corner solution.

[6] Laux (2008) also allows for the possibility of replacing the manager, but the manager's human capital is unobservable so that the analysis does not incorporate the entrenchment associated with observable SHC in our model. Arya, Glover, and Sunder (1998) show how earnings management, although not in the interests of owners *ex post* because it reduces the information available for firing decisions, *is* in shareholders' interests *ex ante* when inducing the manager to exert firm-specific effort. Again, our observable SHC leads to different implications.

[7] Laux and Mittendorf (2011) do not consider an interior optimum for asymmetric information or the effects of SHC on managerial replacement. They do consider the effect of regulations requiring independent directors, which they interpret as reducing the weight on the manager's utility in the board's objective function. In our setting, NYSE and NASDAQ regulations requiring a majority of independent directors may have unintended and complex consequences depending on how this affects the information and control environment. A traditional interpretation might be that independence directly increases board informedness and strengthens the control environment by making the board less liable to capture by management. Plausibly, however, independence could also be associated with lack of in-depth understanding of the firm's operating environment, which could mean *less* board informedness in our model.

enables us to uncover intricate interactions between them, driving the prediction of interior optima for asymmetric information and our "endogenous complementarity" result.

Second, SHC is only subtly associated with the contracting stage, in that SHC does not affect the magnitude of information rents in the optimal contract. Rather, SHC serves an entrenchment role that affords indirect *access* to these rents. Focusing on expropriation by entrenched managers, regulators have concentrated on increasing information flow and control strength to limit concealment and diversion, apparently without considering the indirect links between rents and *ex ante* SHC investment that we identify. Indeed, an artifact of our "win-win" solution is that *ex post* there will be situations where the firm holds up the manager, or the manager receives significant economic rents.

Third, we integrate an additional facet of entrenchment, namely, a direct bias of the board toward managerial preferences when designing the information and/or control infrastructure. We find that this bias interacts with elements of our initial value-maximizing set-up, enhancing our ability to identify circumstances in which regulation of the control environment might increase or decrease firm value.

The next section presents the model. Section III contains the analysis under the basic assumption of shareholder wealth maximization. Section IV extends the analysis to include board objectives that deviate from shareholder wealth maximization, and derives implications for regulation. Section V concludes. Proofs are contained in Appendix A.

II. MODEL

We model a manager's incentive to develop non-contractible firm-specific human capital in order to safeguard his position, ensuring access to future information advantages. The potential value of this access depends on the firm's information and control environment. We characterize this environment along two dimensions: first, "board informedness"—the accuracy with which the board observes and interprets the information available to the manager, and second, the control environment—the extent to which the manager could expect to profit from exploiting his information advantage for private benefit, taking into account the difficulty of diverting hidden value.

Model Set-Up

Timing and Actions

The two-period model has times $t=0$, 1, and 2. In this base case of the model, the board's objective is to maximize shareholder wealth at $t=2$, i.e., cash flow net of compensation payments to the manager and any private benefits he may extract. In Section IV, we consider the case where managerial entrenchment causes modified board objectives.

At $t=0$, the firm (via its board of directors representing shareholders) hires a manager and puts in place the infrastructure that determines the degree of board informedness at $t=1$. During the first period, the manager develops firm-specific human capital $h \geq 0$, which creates firm value gh at $t=2$ *only if* this manager remains employed until $t=2$. If the firm-incumbent relationship terminates before $t=2$, then this sunk investment is lost to both parties. Since h is a *firm-specific* investment, it does not enhance the manager's outside opportunities. And since h is a *human capital* investment, it creates no value for the firm if the manager departs. While h is observable by the board at $t=1$, it is not verifiable, and so wage contracts cannot be written on it. However, the board can terminate the manager's employment at $t=1$. It will rationally do so if firm value is greater with a replacement manager.

At $t=1$, the manager privately and without error observes an exogenous profitability parameter, $i \in \{G, B\}$ ("Good" or "Bad"), with equal probabilities. Relative to the B state, the G

state involves an additional $\phi > 0$ of *hidden* value to be generated during the second period, value that the advantageously informed manager might plausibly seek to appropriate for himself. The profitability state is beyond the control of the manager and could be related to such variables as market conditions, machine productivity, and efficiency of external suppliers, among others. Private observation of i gives the manager his potential information advantage because at $t = 1$, the board observes only an imperfect signal, $\tilde{i} \in \{\tilde{G}, \tilde{B}\}$, the accuracy of which is determined by board informedness.[8]

For the second period, the presence of a manager is necessary to realize terminal cash flows, but the manager has an opportunity to divert some of this value. At $t = 1$, now in possession of its signal $\tilde{i}$, the board has the option to fire the incumbent manager and replace him with a new manager who has *not* observed the profitability parameter i, but has no SHC (i.e., $h = 0$). Also at $t = 1$, the board offers a second-period incentive contract to whichever manager it seeks to employ. Since incentive payments are contingent on verifiable total firm value at $t = 2$, this contract directly motivates the manager's unobservable second-period action choice, namely, the private benefits extracted from the firm, $b \geq 0$. The payoffs to each party are obtained at $t = 2$.

In equilibrium, we will find that the original manager is retained. Therefore, the total verifiable cash flow (i.e., firm value *gross* of the manager's remuneration, but *net* of private benefits) at $t = 2$ is given by:

$$v_i = \begin{cases} v_G \equiv x_G - b_G \equiv gh + \phi - b_G & \text{with probability } \frac{1}{2} \\ v_B \equiv x_B - b_B \equiv gh - b_B & \text{with probability } \frac{1}{2} \end{cases}. \tag{1}$$

To summarize, the value of the investment in SHC is gh, where g is a productivity parameter, and the potential additional value ϕ is exogenously realized when $i = G$. Having observed the realization $i \in \{G, B\}$ and faced with a second-period incentive contract, the manager chooses his second-period private benefits b_i, the incentive-compatible action for each realization of i. Although h, b, and the existence (or not) of ϕ are each unverifiable, v_i *is* verifiable, and so the second-period contract payoffs are based on the realization of v_i.

Manager Utility

The manager's accumulated utility over the two periods is given by:

$$U_0 = w - \gamma h + \beta(b) \tag{2}$$

where w is the monetary compensation paid at t_2, γh is the disutility (cost) of developing SHC in the first period, and $\beta(b)$ is the utility of the second-period private benefits extracted, where:

$$\beta(b) = b - \frac{1}{2}b^2. \tag{3}$$

Note the quadratic $\beta(b)$ is monotonically increasing only on $b \in [0, 1)$, which will make necessary a parameter restriction later in the model.

The manager's first-period SHC investment h is costly to him, but increases gross firm value *only if* the manager stays for the second period. Such investment distinguishes him at $t = 1$ from the potential replacement, who has $h = 0$ and does *not* observe the profitability parameter i. Finally, the

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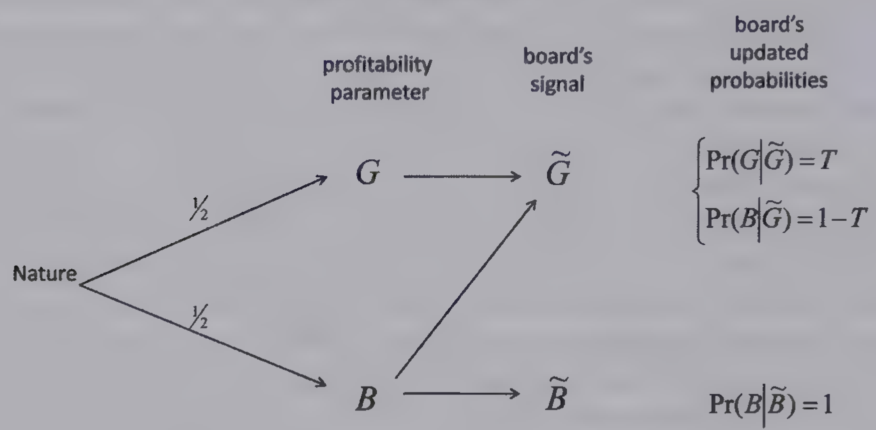
[8] It is important for our results (and in the interests of shareholders *and* the manager) that the manager supplies his SHC *before* he has sight of the $\{G, B\}$ outcome. It is only preemptive observable SHC that can credibly serve to entrench the manager and contribute to the "win-win" outcome we derive. Plausibly, but not necessary for our results, accumulated SHC may also give the manager mastery of the firm-specific systems necessary to exploit realized information advantages.

FIGURE 1
Board Informedness, T



incumbent manager (or any potential replacement) has an alternative outside opportunity, available at $t = 1$, which can provide utility of $\bar{U}_1$.

Board Informedness

The information structure that determines the board's informedness is illustrated in Figure 1. At $t = 0$, the board and manager have common priors, namely, that $\Pr G = \Pr B = \frac{1}{2}$. At $t = 1$, while the manager observes perfectly the realization of i, the board sees only the noisy signal $\tilde{i} \in \{\tilde{G}, \tilde{B}\}$. As shown in Figure 1, if the true state is G, then the board's signal is $\tilde{G}$ with probability 1, but if the true state is B, then the signal may be $\tilde{B}$ or $\tilde{G}$, each with positive probability. Accordingly, observing the signal enables the board to form updated posterior probabilities as follows:

$$\Pr(G|\tilde{G}) = T; \quad \Pr(B|\tilde{G}) = 1 - T; \quad \Pr(B|\tilde{B}) = 1; \quad \Pr(G|\tilde{B}) = 0,$$

where $T \in [\frac{1}{2}, 1]$ is the "truthfulness" or reliability of a $\tilde{G}$ signal.[9] Thus, T is our measure of the accuracy of the board's information, and our (inverse) measure of asymmetric information.

At one extreme, $T = \frac{1}{2}$ and the board's signal is always $\tilde{G}$, and so is entirely uninformative and leaves the board unable to update its priors from $\Pr G = \Pr B = \frac{1}{2}$. At the other extreme, $T = 1$ and the board's signal is perfectly revealing, and so leaves the board symmetrically informed with the manager. Between the two extremes, $T \in (\frac{1}{2}, 1)$, specifying the extent to which the board will be able to update its priors following a $\tilde{G}$ signal.

The unconditional probabilities of the board's possible signals are given by $\Pr\tilde{G} = \frac{1}{2T}$ and $\Pr\tilde{B} = 1 - \frac{1}{2T}$. Effectively, a higher T makes a $\tilde{G}$ signal *less* likely (by creating fewer "false positives"), but increases its information content (*because* there are fewer false positives). A higher T makes the perfectly revealing $\tilde{B}$ signal *more* likely (makes symmetric information more likely).

[9] To simplify the exposition, we have just one of the two possible signals being imperfect. Permitting the $\tilde{B}$ signal to also be imperfect (i.e., $\Pr(G|\tilde{B}) > 0$) complicates the analysis without giving qualitatively different results. The existence of "bias" in the information system may resemble some structures found in the conservatism literature. However, in our model, the board chooses the accuracy of the information system with no ability for the manager to interfere in the generation of the board's signal or in the board's interpretation of that signal. In contrast, the conservatism literature focuses on the incentive to overstate public reports, leading to accounting systems that optimally counter this tendency with conservatism (e.g., Gao 2013).

Anticipating the intuition of what drives our results, the manager's information advantage arises when the board receives the imperfect $\tilde{G}$ signal. In this case, the manager knows whether the true state is G, i.e., whether there exists an additional exogenous value of ϕ (relative to the bad state B). The board's need to make it incentive-compatible for the manager not to conceal and divert this hidden value either gives the manager an information rent R, or permits wastefully high private benefits with inefficiency cost A. Together, these possibilities yield an *expected* cost C if asymmetric information arises in the contracting stage analyzed below.

Given the potential for T to affect the manager's incentives, we make it a parameter of choice, endogenously determined at $t = 0$, and serving as a precommitment to a particular degree of board informedness at $t = 1$. Since the manager is perfectly informed at $t = 1$, board informedness T is the appropriate (albeit inverse) measure of asymmetric information between the manager and the board. We consider that board informedness is determined by (1) the *quality* of information available to the board—the scope, relevance, and precision of data, reports, advice, and other hard and soft information at the disposal of the board to update its priors on the likely outcome of the profitability parameter; and (2) the board's *expertise*—its ability to understand and interpret available information and determine its impact on the posterior probabilities. Important to our model is that the firm commits at $t = 0$ to a particular level of T. Without this commitment, the board would always find it optimal at $t = 1$ to become as fully informed as the manager. Precommitment is achieved by constructing the informational architecture at $t = 0$. This involves appointing a board of appropriate expertise and creating the formal and informal systems necessary to obtain the hard and soft information to be considered at $t = 1$. If such systems are not in place at $t = 0$ to record the information, then we assume that no *ex post* action can be taken to obtain the information at $t = 1$.

It is worth emphasizing that the board is not monitoring the manager's second-period *action;* rather, it is updating on the resolution of the exogenous and independent uncertainty $i \in \{G,B\}$, which is the source of the manager's potential information advantage. The board's signal and consequent posterior probabilities are valuable when designing the optimal compensation contract in the second period.

Control Environment

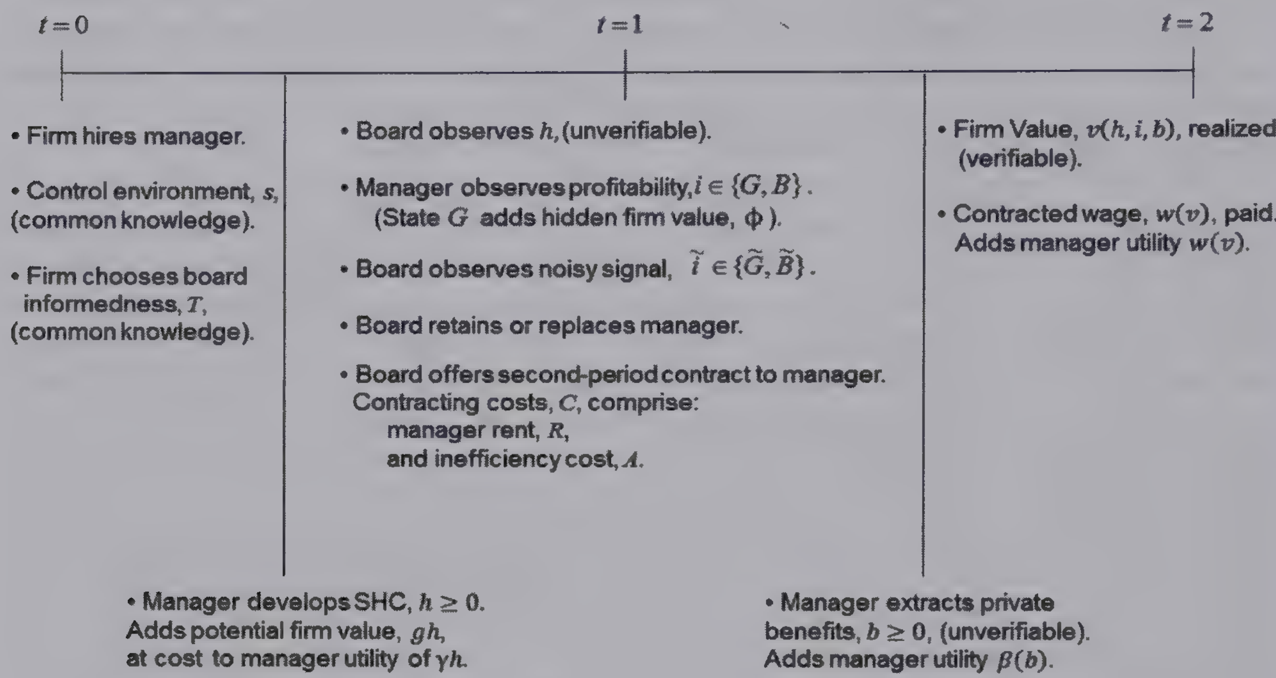
As the final component, we introduce the control strength parameter $s \in [0, 1)$, which reflects the mechanisms that impede concealment and conversion of hidden value to private benefit. Specifically, when the true state is G, the manager could divert the hidden value ϕ from the firm. However, given the costs of manipulation and potential detection associated with the control environment, the manager would realize only $(1 - s)\phi$ of private benefits. Thus, a high s represents a strong control environment (low incentive to divert hidden value) and a low s corresponds to weaker control.

In the present section, we take s as exogenously determined. In Section IV, we extend our model to permit managerial preferences to influence board informedness and, by analyzing the impact on SHC, we study the potentially unintended consequences for firm value of *imposed* changes in s.

Information and Control Environment

It could be argued that our "board informedness" T and "control environment" parameter s share common elements and that some mechanisms could directly affect both. While acknowledging some potential interrelation, an objective of our article is to formally disentangle and distinguish between the two as follows: on one hand, the accuracy T with which the board is able to observe and interpret the manager's information captures the *ex ante* likelihood of an information advantage *existing*—a prerequisite for the manager to have any *opportunity* to divert value into private benefits—and the *magnitude* of that advantage when it does exist. On the other

FIGURE 2
Time Line



hand, s captures those characteristics of the control environment that affect the manager's incentive to divert value *given* that an information advantage exists of a known magnitude and *given* that there is hidden value that presents the opportunity for plausible diversion. This includes features such as the rigor of audit procedures aimed at uncovering any diversion of ϕ *ex post*, probability of detection, and characteristics such as career concerns or likelihood and severity of penalties for diversion (i.e., consequences of detection). This is a subtle, but important, distinction in our analysis. Moreover, our results would be robust even if certain dimensions of s and T were directly linked, providing that some aspects of s remain independent of T.

The notation and sequence of events described above are summarized in Figure 2.

III. ANALYSIS

We solve the model by backward induction. First, we derive the optimal second-period incentive contract offered at $t=1$, conditional on the interim signal $\tilde{i}$ received by the board. The optimal contract specifies wages w, paid to the manager based on final firm values v, and when the board receives the imperfect signal $\tilde{i} = \tilde{G}$, the manager's information advantage either provides an information rent or causes inefficiently high private benefits, and so is costly to the firm. Next, we consider the possibility of replacing the manager at $t=1$, and the incumbent manager's consequent incentive to develop entrenching SHC in the *first* period, conditional on his expected information advantage (captured by T). Finally, we determine the effect of information advantages and the implications for firm value V, to enable us to identify the optimal *ex ante* board informedness T_V^* and its interaction with the control environment s.

Optimal Second-Period Incentive Contract

At $t=1$, and conditional on the realization of signal $\tilde{i}$, the board offers a take-it-or-leave-it employment contract $\{w_{G|\tilde{i}}, w_{B|\tilde{i}}\}$ for the second period. The contract is designed to induce value-

maximizing actions while accounting for the manager's second-period information advantages—i.e., his private benefits extraction b_i and the realization of i (when the board's signal is $\tilde{G}$).

The first-best level of private benefits maximizes firm value from (1), net of wage payment w_i, i.e.:

$$v_i - w_i = x_i - b_i - w_i \tag{4}$$

subject to the manager's reservation utility constraint,[10] $w_i + \beta(b) \geq \bar{U}_1$, i.e.:

$$w_i + b_i - \frac{1}{2}(b_i)^2 \geq \bar{U}_1 \tag{5}$$

which implies a first-best level of benefits where $\beta'(b) = -1$, or $b_{FirstBest} = 0$. In our setting, all private benefit extraction is inefficient.

When the perfectly revealing signal $\tilde{i} = \tilde{B}$ is observed at $t = 1$, the one-to-one relationship between b and $v_B \equiv x_B - b$ then enables the board to induce the first-best $b_{FirstBest} = 0$ with a simple forcing contract, as follows:

Lemma 1: Optimal Contract when the Board Observes $\tilde{i} = \tilde{B}$. When the board observes the fully informative signal $\tilde{B}$, the optimal contract $\{w_{B|\tilde{B}}\}$ is a simple forcing contract that provides the manager's reservation utility and induces the first-best private benefit, $b^*_{B|\tilde{B}} = b_{FirstBest} = 0$.

In contrast, the imperfect signal $\tilde{i} = \tilde{G}$ leads to a second-best outcome. Upon observing $\tilde{G}$, the board updates accordingly, believing that the true state is G with probability T, or is B with probability $1 - T$. The firm rewards only the outcomes $v_G = x_G - b_{G|\tilde{G}} = gh + \phi - b_{G|\tilde{G}}$ or $v_B = x_B - b_{B|\tilde{G}} = gh - b_{B|\tilde{G}}$, with the wages $w_{G|\tilde{G}}$ or $w_{B|\tilde{G}}$, respectively. The incentive contract at $t = 1$ maximizes the firm's conditional expected value, net of expected wage cost:

$$T(gh + \phi - b_{G|\tilde{G}} - w_{G|\tilde{G}}) + (1 - T)(gh - b_{B|\tilde{G}} - w_{B|\tilde{G}}) \tag{6}$$

subject to the manager's reservation utility constraints in (5), and the incentive compatibility constraints for the choice of $b_{i|\tilde{G}}$. The incentive-compatibility constraints are complicated by the hidden value ϕ. In particular, since the manager perfectly observes both the realization i and his own action b, the contract must ensure that it is in the manager's best interest to choose the benefits intended for each realization of i, given his ability to manipulate his action b and obtain the payment intended for the other realization.

In the G state, instead of consuming $b_{G|\tilde{G}}$, the manager could plausibly conceal the true state by consuming the private benefits $b_{B|\tilde{G}}$ intended in the B state, *and* the diverted hidden value ϕ. Diversion of the hidden value ϕ would be subject to the control environment outlined above, and lead to only $(1 - s)\phi$ of private benefits. The incentive-compatibility constraint that deters this diversion is given by:[11]

$$w_{G|\tilde{G}} + \beta(b_{G|\tilde{G}}) \geq w_{B|\tilde{G}} + \beta\Big(b_{B|\tilde{G}} + (1 - s)\phi\Big). \tag{7}$$

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[10] Note that h is already sunk at $t = 1$, so it is not relevant to the incumbent's expected utility at that time. Also, due to the firm-specificity of h, it does not increase his reservation utility $\bar{U}_1$, reflecting the dynamic consistency (hold-up) problem associated with SHC.

[11] We model s as acting only to reduce the manager's benefit from diverting the hidden value ϕ. In equilibrium, the firm gives deliberate contractual inducement for the manager to consume $b_{B|\tilde{G}}$, so any similar control on $b_{B|\tilde{G}}$ would cause a deadweight cost without any benefit to the firm. Indeed, with an endogenous control environment affecting the incentive to consume $b_{B|\tilde{G}}$ and ϕ separately, the differential treatment we employ would arise endogenously.

The board's imperfect signal $\tilde{i} = \tilde{G}$ enables the manager to earn an information rent when the true state is $i = G$, because the right-hand side in (7) strictly exceeds $w_{B|\tilde{G}} + \beta(b_{B|\tilde{G}})$. In equilibrium, the manager receives exactly his reservation utility when $i = B$, so that $w_{B|\tilde{G}} + \beta(b_{B|\tilde{G}}) = \bar{U}_1$, and the concealment (and diversion) opportunity produces an endogenous information rent R (i.e., utility in excess of his reservation level) when $i = G$, given by:

$$R \equiv w_{G|\tilde{G}} + \beta(b_{G|\tilde{G}}) - \bar{U}_1 = \beta\Big(b_{B|\tilde{G}} + (1-s)\phi\Big) - \beta(b_{B|\tilde{G}}). \tag{8}$$

To mitigate the rent in (8), the optimal contract induces a $b_{B|\tilde{G}}$ greater than the first-best. This reduces the rent because the manager has declining marginal utility of benefits ($\beta(b)$ is concave), so that the incentive to divert ϕ into additional benefits decreases with $b_{B|\tilde{G}}$. However, increasing $b_{B|\tilde{G}}$ above first-best inevitably creates an inefficiency cost A (i.e., excess of its cost to the firm versus its utility to the manager) when $i = B$, given by:

$$A \equiv b_{B|\tilde{G}} - \beta(b_{B|\tilde{G}}) = \frac{1}{2}(b_{B|\tilde{G}})^2. \tag{9}$$

Thus, there are two components to the cost of the information asymmetry arising when $\tilde{i} = \tilde{G}$, which together produce a conditional *expected* information cost C, given by:

$$C \equiv T \cdot R + (1-T) \cdot A. \tag{10}$$

At $t = 1$, conditional on the imperfect signal $\tilde{G}$, the value-maximizing board chooses the incentive compatible $b_{B|\tilde{G}}$ to minimize the expected cost C. This cost-minimizing $b_{B|\tilde{G}}$ and resulting values of R, A, and C are presented in the following Lemma.

Lemma 2: Optimal Contract when the Board Observes $\tilde{i} = \tilde{G}$. When the board observes the imperfect signal $\tilde{G}$ with board informedness $T = \Pr(G|\tilde{G})$, the optimal contract $\{w_{G|\tilde{G}}, w_{B|\tilde{G}}\}$ is characterized by:

(i) "Second-best" private benefits in the B state:[12]

$$b^*_{G|\tilde{G}} = b_{FirstBest} = 0 \tag{11}$$

$$b^*_{B|\tilde{G}} = \frac{T}{1-T}(1-s)\phi \tag{12}$$

where $b^*_{B|\tilde{G}}$ is increasing in T.

(ii) Inefficiency cost (i.e., relative to the first-best) in the B state:

$$A^* \equiv b^*_{B|\tilde{G}} - \beta(b^*_{B|\tilde{G}}) = \frac{1}{2}(b^*_{B|\tilde{G}})^2 \tag{13}$$

$$= \frac{1}{2}\left(\frac{T}{1-T}(1-s)\phi\right)^2 \tag{14}$$

where A^* is increasing in $b^*_{B|\tilde{G}}$ and, therefore, in T.

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12 A parameter restriction is necessary to ensure that $\beta(b)$ is monotonically increasing. This requires that the argument of $\beta(\cdot)$ never exceeds 1, i.e., requires $b^*_{B|\tilde{G}} + (1-s)\phi \le 1$. Substituting (12), this is ensured by restricting attention to $(1-s)\phi \le 1-T$ or $T \le 1-(1-s)\phi$. When it is important to consider cases where $b^*_{B|\tilde{G}} + (1-s)\phi > 1$, a more general utility function will be necessary.

(iii) Managerial information rents enjoyed in the G state:

$$R^* \equiv \beta\left(b^*_{B|\tilde{G}} + (1-s)\phi\right) - \beta(b^*_{B|\tilde{G}}) \tag{15}$$

$$= \left(1 - \frac{1}{2}\left(\frac{1+T}{1-T}\right)(1-s)\phi\right)(1-s)\phi \tag{16}$$

where R^* is decreasing in $b^*_{B|\tilde{G}}$ and, therefore, in T, so is maximized at $T^*_R = \frac{1}{2}$.

(iv) Expected information costs:

$$C^* \equiv T \cdot R^* + (1-T) \cdot A^* \tag{17}$$

$$= T\left(1 - \frac{1}{2(1-T)}(1-s)\phi\right)(1-s)\phi \tag{18}$$

where:

$$dC^*/dT = R^* - A^* + \frac{\partial C^*}{\partial b^*_{B|\tilde{G}}} \cdot \frac{\partial b^*_{B|\tilde{G}}}{\partial T} \tag{19}$$

$$= R^* - A^* \tag{20}$$

$$= \left(1 - \frac{1}{2}\frac{1}{(1-T)^2}(1-s)\phi\right)(1-s)\phi \tag{21}$$

and

$$d^2C^*/dT^2 < 0. \tag{22}$$

Lemma 2 characterizes the second-period compensation contract offered by a value-maximizing board. Conditional on the imperfect $\tilde{G}$ signal at $t = 1$, the board designs the compensation contract to minimize the firm's expected information costs C by permitting private benefits $b_{B|\tilde{G}}$ to the point where the marginal increase in expected inefficiency equals the marginal decrease in expected rents (until $\partial C/\partial b^*_{B|\tilde{G}} = T \cdot \partial R/\partial b^*_{B|\tilde{G}} + (1-T) \cdot \partial A/\partial b^*_{B|\tilde{G}} = 0$).

Anticipating the analysis of the optimal choice of T below, note that $b^*_{B|\tilde{G}}$ and the minimized expected cost C^* both depend on board informedness T (fixed at $t=0$). Intuitively, as T increases, it is more likely that a $\tilde{G}$ signal is correct—there is a greater probability that a $\tilde{G}$ signal corresponds to the G state, and a smaller probability that a $\tilde{G}$ signal corresponds to the B state (fewer "false positives," captured by $\Pr(B|\tilde{G}) = 1 - T$). Thus, the firm's concern with the inefficiency cost A (arising in state B) decreases, while that with the rent cost R (arising in state G) increases, causing the cost-minimizing private benefit $b^*_{B|\tilde{G}}$ to increase. The magnitude of $b^*_{B|\tilde{G}}$ will be an important factor in our ultimate determination of optimal board informedness, as it affects the rate at which C^* decreases with T. Specifically, as private benefit $b^*_{B|\tilde{G}}$ increases, inefficiency cost A^* increases and rent R^* decreases, so that $dC^*/dT = R^* - A^*$ decreases (i.e., C^* is concave in T).[13]

Despite the conditional expected cost C^* to the firm, information asymmetry has a benefit to shareholders. Specifically, the prospect of expected information rents at $t = 1$ provides the

[13] The concavity of $C^*(T)$ plays an important role in determining the optimal T below. By inspection of (21), dC^*/dT is positive, then negative, on $T \in [\frac{1}{2}, 1 - (1-s)\phi]$. However, we will see later that it is always negative at the optimum.

manager with an incentive at $t = 0$ to develop valuable SHC in the first period, as illustrated next.

Optimal First-Period Investment in Firm-Specific Human Capital

We have identified above the information costs that the firm may face at $t = 1$ due to the information advantage held by the incumbent manager. These costs could potentially lead the board to then replace the incumbent with a new manager who does not possess an information advantage. However, replacing the incumbent is also costly when he has firm-specific human capital. We proceed to show that the synergy associated with firm-specific human capital means that the possibility of replacement gives the manager incentives to develop SHC in the first period, to protect his position and the accompanying information rent.

Since the board has full discretion over employment contracts, replacement occurs when it is in shareholders' interests at $t = 1$, i.e., when replacement would increase $t = 1$ firm value. If the board observes $\tilde{B}$, then information asymmetry is resolved, so there is no question of replacement. However, if the board observes the imperfect signal $\tilde{G}$ so that shareholders face expected information costs, then the board will base its retention decision on whether the incumbent manager has developed sufficient SHC to offset these costs.

Lemma 3: Firm value when $\tilde{G}$ is observed and incumbent manager is retained. Given the board's signal $\tilde{G}$ and the SHC h invested by the incumbent manager, the conditional expected firm value at $t = 1$ is:

$$V_{t=1|\tilde{G},retain} = gh + T\phi - \bar{U}_1 - C^*. \tag{23}$$

With retention, the returns to h are certainly realized, the board expects hidden value ϕ to be realized only with probability T, and the optimal contract costs the equivalent of the manager's reservation utility plus the conditional contracting costs from Lemma 2.

In contrast to the incumbent, a replacement manager has no information advantage over the board, so it is convenient to assume he sees the same information as the board. Also, he possesses no SHC, so $h = 0$. The incentive contract offered to a replacement manager under symmetric information is similar to that offered with a perfect signal of i as in Lemma 1, in that first-best actions are induced via a forcing contract that provides the replacement's expected reservation utility. Since the replacement does not know the actual realization of i, he chooses the private benefits $b_{FirstBest} = 0$ to ensure the expected wage payments in the employment contract.

Lemma 4: Firm Value when $\tilde{G}$ is Observed and Incumbent Manager is Replaced. Given the board's signal $\tilde{G}$ and replacement of the manager, there is no SHC and no information cost. The conditional expected firm value at $t = 1$ is:

$$V_{t=1|\tilde{G},replace} = T\phi - \bar{U}_1. \tag{24}$$

Comparing the firm's expected value with the incumbent (23) to the firm's expected value with the replacement (24) provides a straightforward replacement policy for the firm, and consequent *ex ante* SHC investment strategy for the manager, as follows:

Lemma 5: SHC investment and the replacement decision when $\tilde{G}$ is observed:

(i) If $gh \geq C^*$, then the board retains the incumbent at $t = 1$. Otherwise, if $gh < C^*$, then the firm replaces the incumbent.

(ii) At $t = 0$, since SHC is costly, if the manager chooses $h < \frac{C^*}{g}$, then he will choose $h = 0$ and attain his reservation utility $\bar{U}_1$. However, if he chooses $h \geq \frac{C^*}{g}$, then he will choose exactly the minimum amount, $h^* = \frac{C^*}{g}$, of SHC to avoid firing, giving the manager's $t = 0$ expected utility:

$$U_0 = \bar{U}_1 - \gamma h^* + \frac{1}{2} R^*. \tag{25}$$

From (25), investment in SHC is incentive-compatible for the manager if the cost of supplying h^* is not greater than the expected information rents it will secure, i.e., $\gamma h^* \leq \frac{1}{2} R^*$. Effectively, the individual rationality constraint $U_0 \geq \bar{U}_1$ places an upper bound $\bar{\gamma}$ on the cost parameter γ for SHC, meaning that γ has to be small enough (relative to its productivity parameter g) for the prospect of rents to motivate the manager to invest in SHC, i.e., $\frac{\gamma}{g} \leq \frac{R^*}{2C^*}$.

Proposition 1: Optimal *ex ante* investment in SHC and contingent firm value:

(i) If the cost parameter, γ, for SHC is greater than $\bar{\gamma} = \frac{gR^*}{2C^*} > 0$ (a closed-form expression for which is given in the proof), then the manager supplies no SHC.

(ii) If $\gamma \leq \bar{\gamma}$, then the prospect of information rents is sufficient to motivate the manager to supply SHC of:

$$h^* = \frac{C^*}{g}. \tag{26}$$

This SHC ensures retention whether $\tilde{G}$ or $\tilde{B}$ is observed, and so gh^* is realized in either case, although the firm incurs offsetting information costs C^* when $\tilde{G}$ is observed. Conditional on the realization of the signal $\tilde{i}$, expected firm value is given by:

$$V_{t=1|\tilde{B}} = gh^* - \bar{U}_1 \tag{27}$$

$$V_{t=1|\tilde{G}} = gh^* + T\phi - \bar{U}_1 - C^* = T\phi - \bar{U}_1. \tag{28}$$

We restrict our analysis to the case where $\gamma < \bar{\gamma}$. Also, since only the $\tilde{G}$ signal involves asymmetric information and investment in sufficient SHC is incentive-compatible, the manager is always retained in equilibrium.

A contribution of our article is to show how the prospect of information rents and the threat of replacement together provide an incentive for the incumbent manager to develop entrenching SHC during the first period. Since SHC is developed *ex ante*, whereas information asymmetries depend on the realization of the *ex post* signal $\tilde{i}$, SHC sometimes provides more than enough synergy to secure the incumbent's position and so creates value captured by the firm. For this reason, the classical hold-up problem is mitigated in a manner that allows *both* parties to expect to benefit from the investment in SHC and from asymmetric information. Specifically, there are no information costs when $\tilde{B}$ is realized. Here, the firm's bargaining power enables it to extract the synergy of SHC at $t = 1$, so the manager incurs the cost of SHC while the firm receives the benefit. In contrast, when the imperfect $\tilde{G}$ is observed, the manager's expected information rent exceeds his cost of developing the SHC, so the manager expects to benefit. We proceed to show how this interaction between the information environment and the value of SHC leads to an optimal level of information asymmetry.

Firm Value and Optimal Information Asymmetry

We now consider the case where board informedness T is determined endogenously at $t = 0$ to maximize shareholder wealth. Expected firm value at $t = 0$, before the realization of $\tilde{i}$, is given by:

$$V = V_{t=1|\tilde{B}}\Pr\tilde{B} + V_{t=1|\tilde{G}}\Pr\tilde{G}. \tag{29}$$

Recall that if $T = \frac{1}{2}$, then $\Pr\tilde{B} = 0$ and $\Pr\tilde{G} = 1$, so the board receives no meaningful update at $t = 1$. However, as T increases, $\Pr\tilde{B} = 1 - \frac{1}{2T}$ increases and $\Pr\tilde{G} = \frac{1}{2T}$ decreases, and the board is able to condition the incentive contract on $\tilde{i}$ in a way that decreases information costs. Specifically, it minimizes C when $\tilde{i} = \tilde{G}$ is observed and eliminates information costs entirely when $\tilde{i} = \tilde{B}$, as in Lemmas 1 and 2. Substituting the conditional firm values from Proposition 1 (with $gh^* = C^*$) into (29) yields:

$$V = gh^* - C^*\Pr\tilde{G} + \frac{\phi}{2} - \bar{U}_1 \tag{30}$$

$$= gh^*\Pr\tilde{B} + \frac{\phi}{2} - \bar{U}_1. \tag{31}$$

Equation (31) illustrates that shareholder value is driven by the product of the value gh^* of the SHC investment motivated by *potential* contracting costs C^*, and the probability $\Pr\tilde{B}$ of ultimately avoiding those costs. The opposing effects of T on $h^* = \frac{C^*}{g}$ and $\Pr\tilde{B}$, respectively, lead to an optimal level of *ex ante* board informedness T_V^* (where the subscript denotes that the objective is V-maximization), as follows:

Proposition 2: Optimal information asymmetry:

(i) Firm value

$$V = \left(T - \frac{1}{2}\right)\left(1 - \frac{1}{2(1-T)}(1-s)\phi\right)(1-s)\phi + \frac{\phi}{2} - \bar{U}_1, \tag{32}$$

is concave in T and reaches an internal maximum at the optimal level of board informedness given by:

$$T_V^* = 1 - \frac{1}{2}\sqrt{(1-s)\phi}. \tag{33}$$

(ii) Board informedness T_V^* optimally increases with control environment strength s.

Increasing board informedness T has the benefit of *improving* the likelihood of observing the perfect $\tilde{B}$ signal, thereby avoiding contracting costs entirely. As T increases, however, dC^*/dT decreases as in Lemma 2 and, eventually, $dC^*/dT < 0$, so that increasing T also reduces SHC $h^* = \frac{C^*}{g}$. The value-maximizing board informedness balances these effects, optimally trading off the magnitude of the SHC motivated by *potential* contracting costs C^* against the likelihood of avoiding contracting costs altogether.[14]

In part (ii) of Proposition 2, we emphasize that there is a complementary relationship between exogenous control strength s and optimal board informedness T_V^*, reflecting that a stronger control environment indirectly decreases the marginal cost of board informedness to the

[14] Expression (33) for T_V^* and the parameter restriction $(1-s)\phi \leq 1 - T$ in footnote 12 together require that $(1-s)\phi \leq \frac{1}{2}\sqrt{(1-s)\phi}$, i.e., $(1-s)\phi \leq \frac{1}{4}$, on which we focus. Recall from Proposition 1 that we focus on $\gamma < \bar{\gamma}$, deriving from the participation constraint $\frac{\gamma}{g} < \frac{R^*}{2C^*}$. This constraint could also be interpreted as placing an upper bound on T, which, were it to bind, would still imply an imperfectly informed board with board informedness again increasing with control strength (this is implicit from the effects of T and s on $\bar{\gamma}$ in the proof of Proposition 1). By restricting our analysis to $\gamma < \bar{\gamma}$, we avoid boundary solutions, and since $\bar{\gamma} > 0$, there always exists γ sufficiently small such that an unbounded (interior) T_V^* exists.

firm. This result follows from the anticipated effect of s on the optimal contracting problem when the imperfect $\tilde{G}$ signal is observed (as characterized in Lemma 2). To see this, recall that the dynamically consistent contract induces the level of private benefits $b_{B|\tilde{G}}$ that minimizes the expected information costs C (the sum of expected information rents and inefficiency costs), yielding $b^*_{B|\tilde{G}} = \frac{T}{1-T}(1-s)\phi$. A stricter control environment (a higher s) reduces the manager's incentive to divert hidden value ϕ and, therefore, decreases the private benefits $b^*_{B|\tilde{G}}$ employed to limit his information rent $R^* \equiv \beta(b^*_{B|\tilde{G}} + (1-s)\phi) - \beta(b^*_{B|\tilde{G}})$. The board anticipates this reduction in private benefits $b^*_{B|\tilde{G}}$ when choosing the value-maximizing information structure T. Specifically, it recognizes that, *ceteris paribus*, the reduction in $b^*_{B|\tilde{G}}$ will imply a lower inefficiency cost A^* (because $b^*_{B|\tilde{G}}$ is closer to the first-best level; Lemma 2(ii)) and a higher information rent R^* (because reducing $b^*_{B|\tilde{G}}$ increases the marginal utility of additional benefits and, therefore, the rent needed to dissuade diversion; Lemma 2(iii)). These effects combine to increase $dC^*/dT = R^* - A^*$ (which is negative at the optimum) and, therefore, reduce the rate at which SHC $h^* = \frac{C^*}{g}$ (and, thus, firm value) decreases with T.[15] Effectively, therefore, a stricter control environment decreases the marginal cost of T to the firm, so that shareholders optimally increase board informedness.[16]

IV. BOARD OBJECTIVES AND REGULATION OF THE CONTROL ENVIRONMENT

Rent extraction, managerial entrenchment, and deviations from shareholder objectives are at the heart of the regulatory debate over corporate control systems. As such, it is worth emphasizing that even when shareholder wealth maximization is the board's goal, our analysis above identifies, first, SHC as a means by which the manager can protect his position (and rents) in a *value-increasing* form of entrenchment and, second, unique interaction between the firm's optimal information structure and its control environment.

We now extend our model to encompass board objectives that *diverge* from shareholder wealth maximization. We incorporate "board capture" by modifying the board's objective function to a simple weighted average of shareholder wealth V and *ex ante* manager utility U_0, similar to Drymiotes (2007), Kumar and Sivaramakrishnan (2008), and Laux and Mittendorf (2011). Analyzing an environment where this divergence in board objectives may not be totally legislated away, we take the extent of board capture as exogenously given and demonstrate that it is pivotal in determining whether regulation of control strength would create or destroy shareholder value.

Formally, the partially captured board is now assumed to choose its information structure T, to maximize the expected value of the objective:[17]

$$Q = mU_0 + (1-m)V. \tag{34}$$

[15] This combined effect of increasing s also incorporates the direct negative effect on R^*, which is dominated by the effect of decreasing $b^*_{B|\tilde{G}}$ on both R^* and A^*, so that dC^*/dT is less negative.

[16] For simplicity, our model assumes "diffuse" priors $\Pr G = \frac{1}{2} = \Pr B$. If, instead, we let $\Pr G = p \in (0,1)$, so that $T = \Pr(G|\tilde{G}) \in [p, 1]$, then it can be shown that $\Pr\tilde{B} = 1 - \frac{p}{T}$, so that $T^*_V = 1 - \frac{1}{2}\sqrt{2(1-p)(1-s)\phi}$ is increasing in p. The more general priors have no direct effect on A^*, R^*, or C^*, but act indirectly through T^*_V so that $gh^* = C^*(T^*_V)$ is indirectly decreasing in p. Hence, an increase in p would decrease the investment in SHC, both absolutely and relative to the exogenous intrinsic component $p\phi$ of firm value (itself increasing in p through the increased likelihood of realizing ϕ). Conversely, a lower p would increase the magnitude and relative importance of the SHC necessary to ensure retention. Our results continue to obtain with this generalization, although the reduction in the manager's *ex ante* expected rents at more extreme values of p tightens the range of parameter values for which SHC can be successfully motivated (captured by $\gamma < \bar{\gamma}$ in Proposition 1).

[17] We restrict consideration to $m \in [0, \frac{1}{3}]$. Our framework does admit values of m greater than $\frac{1}{3}$, although this complicates the analysis significantly without adding significant insights.

When $m=0$, we are in the original shareholder wealth maximization paradigm where Proposition 2 characterizes the value-maximizing information accuracy, $T=T_V^*$. When $m>0$, positive weight is put on manager utility and the board adopts a *less* informative structure, as follows:

Lemma 6: Optimal board informedness under capture. The board's modified objective Q is concave in T and attains an interior maximum at the optimal board informedness corresponding to:

$$T_Q^* = 1 - \frac{1}{2} Z\sqrt{(1-s)\phi}, \tag{35}$$

where $Z \geq 1$ is an increasing function of m, and so T_Q^* is decreasing in m. Also, Z is independent of s, so T_Q^* is increasing in s. The manager's utility is maximized at $T_{U_0}^* = \frac{1}{2}$, and so we have $T_{U_0}^* < T_Q^* \leq T_V^*$.

The "Q-maximizing" board informedness implemented by a captured board lies between that which maximizes manager utility and that which maximizes shareholder value. As shown in the proof, U_0 is monotonically decreasing in T, since the reduction in rents associated with board informedness outweighs the corresponding reduction in the manager's personal cost of SHC investment. Thus, the manager's preferred board informedness would be the one that maximizes his information advantage and expected utility, namely, $T_{U_0}^* = \frac{1}{2}$. Board capture leads to a lower informedness than T_V^* because m biases T_Q^* down toward the manager's preferred level. This implies higher conditional contracting costs C^* and, consequently, an increase in SHC h^*, but an overall reduction in shareholder value. In addition, Lemma 6 illustrates that the complementary relationship between the strength of the control environment and optimal board informedness of Proposition 2 carries over to the more general case with board capture.

Having established the relationship between board informedness, the control environment, and board capture, we turn our attention to preferences for the exogenous control parameter s. Under board capture ($m>0$), board informedness T_Q^* is chosen to maximize a combination of manager utility and shareholder value. Proposition 3 illustrates how manager utility U_0, shareholder value V, and board objective Q would be maximized at three *distinct* strengths of control environment: $s_{U_0}^*$, s_V^*, and s_Q^*, respectively, preferred by management, shareholders, and the board.

Proposition 3: Preferences for the control environment. When the board chooses T_Q^* as in (35), given exogenous s, then shareholder value V, manager utility U_0, and board objective Q vary with s, as follows:

(i) Shareholder value V is concave in s and attains its internal maximum at s_V^*, where s_V^* is increasing in m.

(ii) Manager utility U_0 is concave in s and attains its internal maximum at $s_{U_0}^*$, where $s_{U_0}^*$ is decreasing in m.

(iii) Board objective Q is concave in s and attains its internal maximum at s_Q^*, such that:

$$\begin{array}{lll} s_V^* \leq s_Q^* \leq s_{U_0}^* & \text{for} & 0 \leq m \leq \bar{m} \\ s_{U_0}^* \leq s_Q^* \leq s_V^* & \text{for} & \bar{m} \leq m. \end{array} \tag{36}$$

At $m=0$, the lowest feasible control strength s would be preferred by shareholders. This is because board informedness $T_Q^* = T_V^*$ is already freely chosen solely to maximize shareholder value, optimally trading off the magnitude of the SHC motivated by potential contracting costs C^* against the probability of avoiding those costs, as in Proposition 2. Strengthening control would further decrease C^*—and, thus, the manager's incentive to invest in SHC—without improving the

probability of shareholders avoiding those costs, and so would only diminish firm value. In contrast, the manager prefers, *ceteris paribus*, to develop less SHC and, therefore, would prefer a stronger control environment s, to the point where his first-order condition $-\gamma \frac{dh^*}{ds} + \frac{1}{2}\frac{dR^*}{ds} = 0$ (from (25)) implies that his marginal utility from the reduction in SHC cost γh^* equals his marginal disutility from the reduction in his expected rents.[18] Of course, at $m = 0$, the board cares only about shareholder value and so also would prefer $s_Q^* = s_V^* < s_{U_0}^*$. Indeed, for sufficiently low levels of board capture $m < \bar{m}$, the manager prefers stronger control than do shareholders, i.e., $s_{U_0}^* > s_V^*$.

As m increases—decreasing board informedness T_Q^* below T_V^*—the shareholders' preferred value of s increases. This is because an increase in control strength would now have an additional benefit to shareholders, thanks to the complementarity between T_Q^* and s in Lemma 6, by causing the board to increase T_Q^* back toward the shareholder value-maximizing level T_V^*. In contrast, the manager's preferred value of s decreases with m because the decrease in T_Q^* reduces the rent-mitigating private benefit $b_{B|\bar{G}}^*$, increasing his marginal utility for diverted value and so his aversion to s. Ultimately, for sufficiently high levels of board capture $m > \bar{m}$, the manager prefers weaker control than do shareholders, i.e., $s_{U_0}^* < s_V^*$.

The monotonicities of s_V^* and $s_{U_0}^*$ and their relative magnitudes in Proposition 3 neatly describe the *relative* preferences (for control environment strength) of shareholders, management, and a captured board. Of course, the impact on these stakeholders of any regulated *change* in the control environment depends on the preexisting control environment. To fix ideas, consider the case where an absence of regulation would see the control environment determined by the preferences of a partially captured (i.e., Q-maximizing) board, namely, $s = s_Q^*$. Then Proposition 3 characterizes the circumstances in which shareholders and managers might gain or lose from regulation.

With significant managerial influence $(m > \bar{m})$, the captured board's preferred control environment is weaker $(s_Q^* < s_V^*)$ than that which would maximize shareholder value. Any regulation that forced the board to strengthen the environment toward s_V^* would see a shareholder benefit, constraining a captured board of directors who, in this scenario, are excessively focused on providing managerial rents.

Conversely, at low levels of board capture $(m < \bar{m})$, we can see that managerial influence (albeit limited) already makes the board's preferred control environment stronger $(s_Q^* > s_V^*)$ than that which would maximize shareholder value. Any further strengthening of the control environment (above s_Q^*) would reduce shareholder value to the presumably unintended benefit of manager utility.

These results suggest that regulators should exercise caution when mandating a stricter control environment across all firms. To the extent that shareholders are vulnerable to management capturing boards and permitting a control environment weaker than that which would maximize firm value, the imposition of stricter control standards by a regulator can help correct this market failure. But when managers do not enjoy excessive self-interested influence over the board's choices, such regulation may actually destroy shareholder value. Legislation that seeks to affect aspects of the information and control environment of the firm should be mindful of the inter-relationships between such dimensions and their subtle, yet potentially important, influence on investments in intangibles, such as firm-specific human capital.

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[18] While the manager unambiguously prefers the lowest possible T (due to the dominant effect on his rents, as in Lemma 6, and this irrespective of the value of m), he prefers an interior value of s. This reflects a further subtle difference between T and s, namely, their effects on expected rents $R^* \equiv \beta(b_{B|\bar{G}}^* + (1-s)\phi) - \beta(b_{B|\bar{G}}^*)$. Increased T affects his rents negatively and only indirectly through $b_{B|\bar{G}}^*$, whereas increased s affects rents positively and indirectly through $b_{B|\bar{G}}^*$ and negatively through diverted value $(1-s)\phi$.

V. CONCLUSION

We present a model where the firm's information and control environment indirectly affect the manager's incentive to develop SHC.

Our principal-agent model balances two incentive conflicts (rent protection by managers versus hold-up by shareholders) through the likelihood and extent of information asymmetry, the strength of the firm's control environment, and the key role of managerial replacement decisions. Under the assumed objective of shareholder wealth maximization, we show how committing to limit the information available to the board when setting the manager's compensation contract can increase the manager's incentive to develop entrenching SHC, to the shared benefit of the manager and shareholders. We also show that board informedness optimally increases with the strength of the control environment. This complementarity arises endogenously in our model and reflects the interaction between information costs and SHC.

Our extended analysis includes a dimension of managerial entrenchment envisaging direct influence over the board's objective function. We show how exogenous strengthening of the control environment can increase (decrease) shareholder value when this influence is sufficiently high (low). Our examination uncovers complex interactions between asymmetric information, the control environment, and the incentives for managers to invest in firm-specific human capital. Therefore, our conclusion, that the information and control environment "matters" for investment in SHC, counsels caution on regulators who might employ a one-size-fits-all approach to legislating that environment.

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APPENDIX A

Proofs of Lemmas and Propositions

Proof of Lemma 1

Conditional on the board observing $\tilde{i} = \tilde{B}$, the optimal contract maximizes (4) subject to (5) (with $i = B$) and the incentive compatibility constraint for b_B, namely $w(v(x_B,b_B)) + \beta(b_B) \geq w(v(x_B,b)) + \beta(b)$, where $x_B \equiv gh$. The solution is a simple forcing contract with payments given by:

$$w(v_B) = \begin{cases} \bar{U}_1 & \text{if } v_B = gh - b_{FirstBest} \\ 0 & \text{otherwise.} \end{cases} \tag{37}$$

Given this contract (and abstracting from the already-sunk cost of h), the manager's utility is:

$$U_1 = \begin{cases} \bar{U}_1 & \text{if } v_B = gh - b_{FirstBest} \\ B(b) & \text{otherwise,} \end{cases} \tag{38}$$

which satisfies incentive compatibility since $\beta(b)$ is maximized at $\beta(1) = \frac{1}{2}$, and we assume $\bar{U}_1 \geq \frac{1}{2}$. ■

Proof of Lemma 2

Conditional on the board observing $\tilde{i} = \tilde{G}$, the optimal contract maximizes:

$$T(gh + \phi - b_G - w_G) + (1 - T)(gh - b_B - w_B) \tag{39}$$

(where $w_i \equiv w(v_i)$), subject to the participation constraints:

$$w_G + b_G - \frac{1}{2}(b_G)^2 \;\geq\; \bar{U}_1 \tag{40}$$

$$w_B + b_B - \frac{1}{2}(b_B)^2 \;\geq\; \bar{U}_1 \tag{41}$$

and the additional incentive compatibility constraints:

$$w_G + b_G - \frac{1}{2}(b_G)^2 \;\geq\; w_B + \Big(b_B + (1-s)\phi\Big) - \frac{1}{2}\Big(b_B + (1-s)\phi\Big)^2 \tag{42}$$

$$w_B + b_B - \frac{1}{2}(b_B)^2 \;\geq\; w_G + \Big(b_G + (1-s)\phi\Big) - \frac{1}{2}\Big(b_G + (1-s)\phi\Big)^2. \tag{43}$$

Since the right-hand side of (42) strictly exceeds the left-hand side of (41), the manager receives strictly more than $\bar{U}_1$ when $i = G$, so (40) is *not* binding.

The concavity of $b - \frac{1}{2}b^2$ implies that only (42) (and *not* (43)) will bind, so that the optimal contract maximizes (39) subject to (41) and (42).

Constructing the Lagrangian and calculating First-Order Conditions gives (12) to prove part (i). Parts (ii), (iii), and (iv) follow by direct substitution. ■

Proof of Lemma 3

To simplify, we put all the compensation weight on the outcome $i = G$. The solution is then a forcing contract with payments given by:

$$w(v_G) = \begin{cases} \frac{1}{T}\bar{U}_1 & \text{if } v_G = \phi \\ 0 & \text{otherwise.} \end{cases} \tag{44}$$

Given this contract, the manager's utility is:

$$U_1 = \begin{cases} \frac{1}{T}\bar{U}_1 & \text{if } v_G = \phi (\text{probability } T) \\ 0 & \text{otherwise (probability } 1-T) \end{cases} \tag{45}$$

and his expected utility is exactly $\bar{U}_1$. ■

Proof of Lemma 4

(i) Comparing (23) and (24), $V_{t=1|\tilde{G},retain} \geq V_{t=1|\tilde{G},replace}$ if and only if $gh \geq C^*$.
(ii) U_0 in (2) is strictly decreasing in h for $h \geq h^*$. ■

Proof of Proposition 1

From Lemma 5, if the manager supplies any SHC, then he supplies $h^* = \frac{C^*}{g}$, giving expected utility (from (25)) of $U_0 = \bar{U}_1 - \gamma\frac{C^*}{g} + \frac{R^*}{2}$. This is incentive-compatible if and only if $U_0 \geq \bar{U}_1$, i.e., for $\frac{\gamma}{g}$ not greater than $\frac{\frac{1}{2}R^*}{C^*}$ (the ratio of *ex ante* expected rents to conditional expected contracting costs), i.e., for:

$$\gamma \leq \frac{g}{2T}\frac{\left(1 - (1+T)\frac{(1-s)\phi}{2(1-T)}\right)}{\left(1 - \frac{(1-s)\phi}{2(1-T)}\right)} = \bar{\gamma} < g. \tag{46}$$

By inspection or direct differentiation, $\bar{\gamma}$ is decreasing in T. Since $T \leq 1 - (1-s)\phi$, this implies that $\bar{\gamma} \geq g\frac{(1-s)\phi}{2(1-(1-s)\phi)} > 0$. By inspection or direct differentiation, $\bar{\gamma}$ is increasing in s.

Part (ii) follows from Lemmas 1 and 3. ■

Proof of Proposition 2

Substitute $\Pr\tilde{B} = 1 - \frac{1}{2T}$ and the expression for h^* from (26) and (17) into (31) to obtain (32). The first- and second-order conditions for the objective (32) identify the solution (33) in part (i).

Part (ii) follows by inspection of (33). ■

Proof of Lemma 6

Substitute h^* from (26), C^* from (18), and R^* from (16) into (25) for the manager's *ex ante* utility U_0, and substitute this and V from (32) into expression (34) for Q. The first- and second-order conditions give solution (35) in the Lemma, with $Z = \sqrt{2-\mu}$, where $\mu = \frac{1-3m}{1-(1+\frac{\gamma}{g})m}$ and we focus on $m \leq \frac{1}{3}$. Since $\gamma < g$, as in (46), we can see that μ is decreasing in m, and so T_Q^* is decreasing in m. Also, $Z \leq \sqrt{2}$, so $T_Q^* \leq T_V^*$.

Finally:

$$\frac{d}{dT}U_0 = -\frac{1}{2}(1-s)\phi\left(2\frac{\gamma}{g} + \frac{1}{(1-T)^2}(1-s)\phi\left(1-\frac{\gamma}{g}\right)\right) < 0. \tag{47}$$

The term $\left(1-\frac{\gamma}{g}\right)$ is positive because $\gamma < g$; hence, U_0 is decreasing in T and is maximized at $T_{U_0}^* = \frac{1}{2}$. ■

Proof of Proposition 3

Let $\underline{s} = 1 - \frac{1-T}{\phi}$ (the minimum parameter restriction on s).

Substitution of $T_Q^*(s)$ from (35) into U_0, as in the proof of Lemma 6, gives $U_0(s)$. Direct differentiation gives the (quadratic) first-order condition, so U_0 has exactly two stationary points that can be found explicitly. To distinguish between them, evaluate $\frac{dU_0}{ds}|_{s=\underline{s}} > 0$ and $\frac{dU_0}{ds}|_{s=1} < 0$. Also, $lim_{s\to\infty} U_0(s) = \infty$, so the continuous function $U_0(s)$ attains a local maximum at some $s_{U_0}^* \in (\underline{s}, 1)$ and a local minimum at some $s^* \in (1, \infty)$. The smaller of the two roots corresponds to $s_{U_0}^*$. By direct differentiation, we see $\frac{d}{dm}s_{U_0}^* < 0$, so $s_{U_0}^*$ is decreasing in m. The derivation of s_V^* to maximize $V(s)$, from (32) and (35), proceeds similarly.

Next, explicit evaluation of $s_V^*(m)$ and $s_{U_0}^*(m)$ gives $s_V^*|_{m=0} < s_{U_0}^*|_{m=0}$, but $s_V^*|_{m=\frac{1}{3}} > s_{U_0}^*|_{m=\frac{1}{3}}$. The continuous functions $s_{U_0}^*$ and s_V^* are, respectively, decreasing and increasing in m, so there exists a unique $\bar{m} \in \left(0, \frac{1}{3}\right)$ such that $s_V^* = s_{U_0}^*$.

Finally, $\frac{d}{ds}Q(s) = m\frac{d}{ds}U_0(s) + (1-m)\frac{d}{ds}V(s)$ is also a quadratic. For $0 \leq m \leq \bar{m}$, we have $s_V^* \leq s_{U_0}^*$, then $\frac{d}{ds}Q(s)|_{s=s_V^*} \geq 0$ and $\frac{d}{ds}Q(s)|_{s=s_{U_0}^*} \leq 0$. Hence, Q attains its maximum at $s_Q^* \in [s_V^*, s_{U_0}^*]$. Conversely, for $\bar{m} \leq m$, we have $s_{U_0}^* \leq s_V^*$, then $\frac{d}{ds}Q(s)|_{s=s_{U_0}^*} \geq 0$ and $\frac{d}{ds}Q(s)|_{s=s_V^*} \leq 0$. Hence, Q attains its maximum at $s_Q^* \in [s_{U_0}^*, s_V^*]$. ■

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The Consequences of Hiring Lower-Wage Workers in an Incomplete-Contract Environment

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ABSTRACT: Firms frequently attempt to increase profits by replacing some existing workers with new lower-wage workers. However, this strategy may be ineffective in an incomplete-contract environment because the new workers may provide lower effort in response to their lower wages, and hiring new lower-wage workers may damage the remaining original workers' reciprocal relationship with the firm. We conduct an experiment to examine this issue and find that when new lower-wage workers become available, firms hire them to replace original higher-wage workers and pay the new workers lower wages. However, these lower wages do not improve firm profit because the decision to hire new lower-wage workers causes both the new and remaining workers to provide lower effort. Moreover, hiring lower-wage workers reduces new workers' payoffs and, thus, decreases social welfare. These unintended consequences suggest that firms should consider both the wage savings and the potential costs when deciding whether to replace some workers with new lower-wage workers. We discuss the implications of our findings for contract design, hiring practices, and managerial accountants.

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I. INTRODUCTION

Motivating workers to provide effort to increase firm profitability is a primary goal of firm owners. Firms use output-based compensation contracts, monitoring systems, and other formal control systems to elicit the desired amount of worker effort. For example, because worker effort is typically not directly observable, firms sometimes provide workers with a well-specified incentive contract that links wages with output so that a higher wage can be earned if output is high (Holmstrom 1979; Lambert 2001). However, firms also frequently employ less-specified incomplete contracts (often fixed-wage contracts) in practice (MacLeod and Parent 1999), either because they are unable to design a more complete contract or because they believe that an incomplete contract will generate as much or more firm profit as a more complete contract (Akerlof 1982; Lazear 1986).[1] Considerable prior research in economics and accounting shows that firms can benefit from offering fixed-wage contracts because firms and workers often engage in gift exchange, in which firms offer a "gift" of a wage greater than the market-clearing level and workers reciprocate with a "gift" of greater effort than the minimum enforceable amount (Fehr, Kirchsteiger, and Riedl 1993; Fehr, Gachter, and Kirchsteiger 1997; Hannan, Kagel, and Moser 2002; Hannan 2005; Kuang and Moser 2009, 2011).

An important strategic option for many firms is the opportunity to hire new workers at lower wages. This opportunity is becoming more frequent because of the increasing ability of firms to move operations to locations with lower-wage workers and workers' ability to quickly move to locations with employment opportunities. In such situations, firm profit will increase if firms use complete-employment contracts and are able to hire new workers at lower wages. However, if firms cannot enforce a complete contract, or choose not to, but instead use incomplete-employment contracts, then the consequences of firms' decisions to hire lower-wage workers are less clear. This is especially true when firms only replace some of their higher-wage workers and retain others. The primary purpose of our study is to examine how the introduction of new lower-wage workers to an existing labor pool affects the behavior of firms and workers.

In our incomplete-contracting setting, new "lower-wage" workers have a lower minimum wage than other workers, and firms offer lower wages to these new lower-wage workers. Standard gift exchange between firms and workers in an incomplete-contracting setting means that workers' effort depends on their wage level. However, the lower minimum wage of the new lower-wage workers could have a separate effect on their effort beyond their standard gift-exchange response. Specifically, the effort of the new lower-wage workers could depend on whether they evaluate their lower wage relative to their minimum wage.

If lower-wage workers evaluate their wage offer relative to their lower minimum wage, then they could provide the same level of effort for their lower wage as other workers with a higher minimum wage provide for their higher wage. This is a possibility because the perceived gift in the wage of the lower-minimum-wage worker, i.e., the difference between their wage and their lower minimum wage, could be equivalent to the perceived gift in the wage of the higher-minimum-wage worker.[2] Consequently, firm profit would be higher for firms with lower-minimum-wage workers because they pay lower wages while receiving the same amount of effort as firms with higher-

[1] Firms may not be able to design a complete contract if output is not measurable at a reasonable cost and, therefore, is not contractible (Lazear 1986).

[2] For example, the difference between a wage offer of 35 and a minimum wage of 10 (25) is the same as the difference between a wage offer of 45 and a minimum wage of 20 (25).

minimum-wage workers receive for paying higher wages. However, the new lower-wage workers could base their effort on the absolute level of their wage, as in standard gift exchange, rather than on the difference between their wage and their lower minimum wage. In this case, workers will provide lower effort in response to their lower wage offers and firm profit may not increase because the financial benefit of paying lower wages would be offset by the cost of lower effort.

In addition to the effect on the new workers' effort discussed above, the effort of the higher-wage workers who remain with the firm could also be affected. Specifically, in an incomplete-contract environment, the remaining higher-wage workers could lower their effort when firms hire new lower-wage workers if they view the firm's action as unfair or as a violation of social norms. Prior research in accounting (Towry 2003; Hannan 2005; Zhang 2008; Kuang and Moser 2009; Tayler and Bloomfield 2011; Christ, Sedatole, and Towry 2012) provides evidence that violations of workers' social norms (e.g., fairness, trust, and reciprocity) affect their behavior. Fehr, Kirchsteiger, and Riedl (1996) and Kuang and Moser (2009, 2011) show that perceived violations of reciprocity can result in lower worker effort. Moreover, Fehr and Fischbacher (2004) provide evidence that individuals react negatively to violations of social norms that harm others even when the violations do not affect them personally. If remaining higher-wage workers lower their effort because they view hiring a new lower-wage worker as unfair or as a violation of a social norm, then this represents a social cost to the firm that would offset the financial benefit of paying lower wages.

Depending on how the hiring of new lower-wage workers affects both the new workers' and remaining workers' effort, the firm's cost savings from paying lower wages to new workers could be partially or fully offset by lower effort from both the new and remaining workers. As a result, firm profit could remain constant, or even decrease, rather than increase as firms might expect as a result of hiring new lower-wage workers.

To examine how the introduction of new lower-wage workers to an existing labor pool affects the behavior of firms and workers in a gift-exchange environment, we conduct an experiment with two experimental conditions. In our low-wage condition, the firm has the option to replace an original worker with a new lower-minimum-wage worker who can perform the same task as the original worker. In our same-wage condition, the firm has the option to replace an original worker with a new worker who has the same minimum wage as the firm's original workers. Our low-wage condition is the setting of primary interest because it reflects the situation firms face when they gain access to a new lower-wage labor pool or when additional workers who are willing to work for lower wages enter the existing labor pool. Although this is our primary setting of interest, we compare firm and worker behavior in this condition to that in the same-wage condition because this allows us to demonstrate that the observed effects of introducing new low-wage workers are not due to the increase in the labor supply that occurs when new lower-wage workers are introduced, but rather reflect the unexpected costs that arise because workers react to the fact that the firm hired a new worker who could be paid a lower wage.

During the first half of our experiment in both conditions, each firm is randomly and anonymously rematched in each period with two workers who have a common labor cost to the firm. The firm decides what wage to offer both workers, and then both workers receiving the wage offer independently decide whether to accept or reject it. Workers who accept the wage offer then decide how much effort to provide. The purpose of these early periods is to allow firms and workers to establish a baseline level of gift exchange between firms and workers.

In the second half of the experiment, each firm is randomly and anonymously rematched in each period with *three* workers, two original workers and one new worker. The firm can still only hire two workers, as was the case in the first half when there were only two workers. Consequently, the firm must decide whether to replace one of the two original workers with the new worker or hire the two original workers. Recall that in the low-wage condition, the new worker has a *lower* minimum wage than the firm's original workers, whereas in the same-wage condition, the new

worker has the *same* minimum wage as the firm's original workers.[3] This is the only difference between the conditions; all other factors are held constant.

We find that a significant portion of firms hire new workers in both conditions, but the frequency of hiring new workers to replace original workers is greater in the low-wage condition than in the same-wage condition. We also find that firms pay lower wages to new lower-wage workers than to new same-wage workers. However, despite paying lower wages, firm profit does not increase as a result of hiring new lower-wage workers for two reasons. First, new lower-wage workers provide less effort than new same-wage workers and, second, the remaining original workers also provide less effort when firms hire new lower-wage workers than when firms hire new same-wage workers. Finally, we find that hiring new lower-wage workers decreases new workers' payoffs and, thus, social welfare decreases as well.

Our findings extend the results of prior studies that investigate the reciprocal relationship between firms and workers (Hannan et al. 2002; Brandts and Charness 2004; Gachter and Thoni 2010; Owens and Kagel 2010) and the effects of social norms in accounting settings (Towry 2003; Hannan 2005; Tayler and Bloomfield 2011; Kuang and Moser 2009, 2011; Christ et al. 2012). An important function of managerial accounting is to evaluate proposed changes in organizations using cost-benefit analysis. We provide evidence that management accountants should not only consider the direct wage savings from switching to lower-wage workers, but also the potential indirect costs that can arise if hiring such workers results in lower effort.

Understanding how firms' contract choices and hiring decisions affect workers' effort can help firms design more effective contracts and employ more successful hiring policies. In particular, recognizing the potential costs of replacing some current workers with new lower-wage workers in an incomplete-contract environment may help firms decide whether to adopt this strategy. If hiring lower-wage workers results in unexpected costs of lower effort that partially or fully offset the benefits of reduced labor costs, then firms may not want to follow this strategy.

Awareness of the unintended costs of lower effort that we observe could also affect the type of employment contract firms choose to offer their workers. Firms that have the ability to enforce relatively complete employment contracts, but instead offer their workers incomplete gift-exchange contracts because of the expected benefits of worker reciprocity, may not benefit as much from this decision as they expect if they subsequently decide to replace some of their current workers with new lower-wage workers. In such cases, the benefits of offering an incomplete gift-exchange contract could be offset by the costs of lower effort associated with hiring lower-wage workers such that the firm would be better off offering a more complete contract.

The next section provides background for our study. Our hypotheses are developed in Section III. Section IV describes our experimental design, Section V reports our results, and Section VI summarizes and discusses our findings.

II. BACKGROUND

Agency theory posits that firms structure contracts to maximize firm profit. These contracts maximize firm profit by paying agents the lowest wage possible (i.e., agents' reservation wage) that induces the agents to provide the desired amount of effort (Baiman 1982, 1990). In complete-contract environments, in which firms can control worker effort (e.g., with a forcing contract), paying lower wages will, by definition, improve firm profit.

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[3] For the remainder of the paper, we refer to new workers with lower minimum wages than original workers as "new lower-wage workers" and new workers with the same minimum wage as original workers as "new same-wage workers."

However, firms often offer workers incomplete contracts (primarily fixed-wage contracts) in practice. Fixed-wage contracts can generate as much firm profit as more complete contracts because of positive reciprocity between firms and workers (Fehr et al. 1993, 1996; Bewley 1999; Gachter and Fehr 2002; Hannan et al. 2002; Hannan 2005; Falk 2007). As discussed earlier, this reciprocal relationship between firms and workers is typically referred to as "gift exchange" (Akerlof 1982). Gift exchange can yield higher firm profit than expected under conventional economic reasoning because it results in increased overall welfare that can be shared by firms and workers.

Of course, fixed-wage contracts do not guarantee a profitable gift-exchange relationship. For this to occur, (1) firms must trust workers to reciprocate with higher effort before they will offer a wage above the market-clearing level, and (2) workers must actually reciprocate a firm's higher wage offer with higher effort.[4] Fixed-wage contracts are incomplete because they do not include explicit performance incentives, but rather allow workers discretion over effort (Fehr et al. 1996). As such, workers could provide low effort because the firm does not trust them to provide high effort and, thus, offers them the low equilibrium wage. In addition, even if firms trust workers to reciprocate their higher wage with higher effort, workers may not provide higher effort because they only want to maximize their wealth or because they view the firm's wage offer as unfair or other actions taken by the firm as violating social norms.

III. DEVELOPMENT OF HYPOTHESES AND RESEARCH QUESTIONS

Firm Behavior When New Lower-Wage Workers Become Available

When firms have the option to replace an existing original worker with a new worker, we predict that they will do so more often and will offer the new worker a lower wage when the new worker has a lower minimum wage than the original workers (low-wage condition) than when the new worker has the same minimum wage as the original workers (same-wage condition). This prediction follows naturally from a model of firms and workers as wealth-maximizers. In this model, firms offer all workers exactly their minimum wage because they anticipate that all workers will respond with the lowest possible effort. Thus, in equilibrium, firm profit is higher when firms hire new workers with a lower minimum wage than when they hire new workers with the same minimum wage.

This prediction is likely to hold even when behavior departs from the assumption of purely wealth-maximizing firms and workers. Prior research finds that workers may use their minimum wage as a reference point for evaluating the amount of gift in their wage offer (Brandts and Charness 2004; Falk, Fehr, and Zehnder 2006; Owens and Kagel 2010). For example, a new same-wage worker with a minimum wage of 20 may perceive a wage offer of 45 as including a gift of 25. However, a new lower-wage worker with a minimum wage of 10 may perceive a lower wage offer of 35 as also including a gift of 25. Therefore, firms may be able to offer lower wages to new lower-wage workers and receive a similar amount of effort, thereby increasing firm profit. If firms anticipate that the new lower-wage workers will evaluate the gift in their wage offers using their lower minimum wage as a reference point, then they may decide to offer these workers lower wages, trusting that they will reciprocate with the same level of effort as workers with a higher minimum wage. Based on the reasoning offered above, our first two hypotheses are:

H1: Firms will hire new lower-wage workers more often than new same-wage workers.

[4] Consistent with Rousseau, Sitkin, Burt, and Camerer (1998, 395), we define trust as "the intention to accept vulnerability based upon positive expectations of the intentions or behavior of another," and consistent with Cox (2004), we define reciprocity as "the in-kind response by a second party who knows with certainty that he has been trusted by the first party."

H2: Firms will make lower wage offers to new lower-wage workers than to new same-wage workers.

New Lower-Wage Workers' Effort

H1 and H2 predict that firms will hire new lower-wage workers more often and pay them lower wages than new same-wage workers. New lower-wage workers' effort will likely depend on the wages they receive because they are interacting with the firm in a gift-exchange setting, i.e., higher wages lead to higher effort and *vice versa*. However, in our setting, there is another factor other than the firm's wage offer that could affect new lower-wage workers' effort.[5]

As discussed in the development of H2, prior research finds that workers sometimes use their minimum wage as a reference point for evaluating the amount of gift in their wage offer (Brandts and Charness 2004; Falk et al. 2006; Owens and Kagel 2010). That is, new workers could perceive the amount of the gift in the firm's wage offer as the difference between their wage offer and their minimum wage, and this perceived "gift" could affect the new workers' effort. If this is the case, then new lower-wage workers could provide more effort than new same-wage workers for any given wage because the perceived size of the gift in any given wage is larger for new lower-wage workers than for new same-wage workers. For example, the gift in a wage offer of 45 is 35 when the minimum wage is 10, but only 25 when the minimum wage is 20.

In a gift-exchange environment such as ours, the type of effort response described above is necessary for firm profit to increase as a result of paying lower wages to the new lower-wage workers. That is, if firms pay lower wages to new lower-wage workers, but these workers do not base their effort on a perceived larger gift in their wage, but instead simply lower their effort in response to their lower wages as in standard gift exchange, then firm profit will not increase as a result of hiring the new lower-wage workers. In this case, the lower effort provided by the new lower-wage workers in exchange for lower wages is a cost to the firm of hiring new lower-wage workers that would offset the financial benefit to the firm of paying lower wages.

Because Brandts and Charness (2004), Falk et al. (2006), and Owens and Kagel (2010) provide evidence that the level of the minimum wage can have a separate effect on worker effort beyond that of the absolute wage level alone as in standard gift exchange, our third hypothesis is:

H3: New lower-wage workers will provide greater effort for a given wage than new same-wage workers.

Remaining Original Workers' Effort

We are also interested in how hiring a new lower-wage worker versus hiring a new same-wage worker affects remaining original workers' effort. While we expect there to be a standard gift-exchange relationship between firms and the remaining original workers, we also expect that this

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[5] Although not discussed in our hypothesis development, there is an additional factor in our setting that could affect worker effort separate from the wage offer alone. Because firms can make different wage offers to their two workers when hiring a new worker, differences in coworkers' wage offers could also affect their effort. Prior evidence is mixed. Gachter and Thoni (2010) and Cohn, Fehr, Herrmann, and Scheider (2012) find that workers provide less effort when they are paid less than a coworker, while Charness and Kuhn (2007) do not find such a difference in a setting in which workers had different productivity rates, presumably because workers believed that the wage differences were justified by the differences in productivity. The difference in minimum wages in our setting could lead new lower-wage workers to believe that their lower wages are justified and, thus, their coworkers' wages would not affect their effort. Nevertheless, we control for any such possible effect in all our tests of worker effort.

relationship will be more adversely affected when firms replace an original worker with a new lower-wage worker than with a new same-wage worker.

Remaining original workers could react negatively to the firm's decision to hire new lower-wage workers if they believe this violates a social norm. Prior research finds that workers perceive it to be unfair for a firm to exploit an increase in its power to take advantage of its workers (Kahneman, Knetsch, and Thaler 1986; Fehr et al. 1993, 1996; Kuang and Moser 2009). Moreover, Fehr and Fischbacher (2004) show that individuals react negatively to violations of social norms that harm others even when the violations do not affect them personally. In our setting, remaining original workers' negative reactions will likely result in lower effort, and this effect is likely to be stronger when firms hire a new lower-wage worker than a new same-wage worker because workers may believe that firms are taking advantage of the new lower-wage worker's lower minimum wage. This lower effort by the remaining original workers would represent a social cost to the firm of hiring a new lower-wage worker that could offset the financial benefit of paying lower wages. Based on this reasoning offered above, our fourth hypothesis is:

H4: Remaining original workers will provide lower effort when firms hire a new lower-wage worker than when firms hire a new same-wage worker.

Firm Profit, Worker Payoff, and Social Welfare

We next consider the overall effect of hiring new lower-wage workers versus hiring new same-wage workers on firm profit, workers' payoffs, and social welfare (the sum of firm profit and workers' payoffs). Any difference in the profit of firms that hire a new lower-wage worker and those that hire a new same-wage worker depends on both the wages they pay to the new and remaining original workers and on the effort these workers provide. Although we predict that firms who hire new lower-wage workers will pay new workers lower wages than firms who hire new same-wage workers, it is nevertheless difficult to predict how hiring a new lower-wage worker will affect firm profit. The effect is ambiguous because H3 predicts higher effort by new lower-wage workers than by new same-wage workers for any given wage, but H4 predicts lower effort by remaining workers for any given wage when a new lower-wage worker is hired than when a new same-wage worker is hired.

The effect of hiring new lower-wage workers versus hiring new same-wage workers on workers' payoffs is also unclear because it depends on both the wages received and the effort provided by the new and remaining original workers. Although we make directional predictions about wages and effort in our hypotheses, we cannot predict the net effect on workers' payoffs because payoffs increase from wages received, but decrease from the cost of effort. Finally, without a prediction for how hiring a new lower-wage worker versus a new same-wage worker will affect either firm profit or workers' payoffs, we are also unable to predict the impact on social welfare. Thus, we pose the following research question:

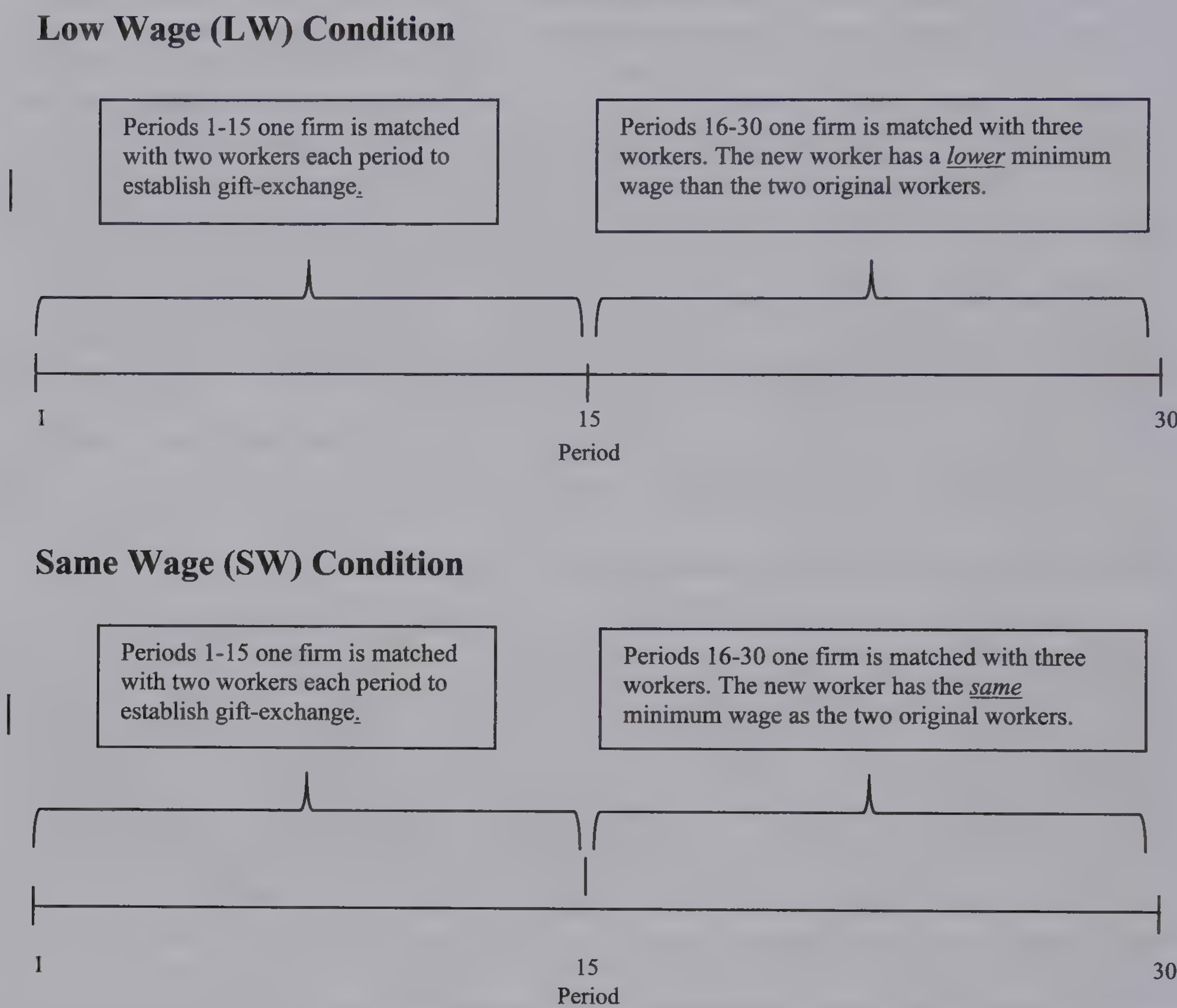
RQ: How does hiring a new lower-wage worker, as compared to a new same-wage worker, affect firm profit, workers' payoffs, and social welfare?

IV. METHOD

Overview

As explained earlier, we are primarily interested in firm and worker behavior when firms can hire a new lower-minimum-wage worker in the second half of the experiment. However, as shown in Figure 1, our experiment consisted of two conditions: Low Wage (LW) and Same Wage (SW).

FIGURE 1
Experimental Design



During the first 15 periods, the procedures are exactly the same in the Low Wage (LW) and Same Wage (SW) conditions. During the first 15 periods, there are nine firms and 18 workers that participate in the labor market in each experimental session. Each firm is randomly matched with two workers each period. Starting with period 16, in both conditions, each of the nine firms is now matched with three workers. The additional worker results in an increase in the labor supply from 18 to 27 workers. In the Low Wage condition, the additional worker can be hired at a lower minimum wage (10) than the two original workers (20). In the Same Wage condition, the additional worker has the same minimum wage (20) as the two original workers.

In both conditions, a session consisted of 30 periods, which were divided into two 15-period halves. In the second half of the experiment, the labor supply increased by one new worker in both conditions. In the LW condition, the new worker entering the market in the second half of the experiment had a lower minimum wage than the original workers, whereas in the SW condition, the new worker entering the market had the same minimum wage as the original workers.

The SW condition controls for the fact that two things change in the second half of the LW condition, i.e., firms can hire a worker with a lower minimum wage and the labor supply increases. Because the labor supply also increases in the SW condition, the only difference between the LW and SW conditions is that firms can hire a new worker with a lower minimum wage in the LW condition. This design allows us to isolate the effect of hiring a new worker with a lower minimum wage by comparing results in the LW condition to those in the SW condition.

One hundred forty-four participants completed our experiment in an experimental economics laboratory using Fischbacher's (2007) z-Tree software. The participants were recruited online from a pool of approximately 1,300 individuals using Greiner's (2004) ORSEE software.[6] We conducted two separate experimental sessions for each condition. Each session had 36 participants, for a total of 144 participants (36 participants × 4 sessions).

During the first half of each experimental session, the procedures were identical for the LW and SW conditions. In both conditions, nine randomly chosen participants did not participate in the first half of the session. These participants were in the lab with all other participants when the experimental instructions were read aloud, but did not observe what happened or receive any feedback regarding the outcomes for the other participants in the first half of the experiment. Rather, they answered trivia questions for money during the first 15 periods. The remaining 27 participants were assigned either to the role of a firm or a worker (nine firms and 18 workers) and kept these assigned roles throughout the session.

During the first half of each session, one firm was randomly rematched with two workers each period. Because firms and workers were randomly rematched each period, participants never knew with whom they were matched during the session.[7] Thus, in some sense, all workers who are hired by a firm in a period are new to that firm for that period. However, this is not what we mean when we describe a worker as a "new worker." Rather, we use the label "new" to describe the group of workers who did not participate in the first half of the experiment, but who then entered the labor pool in the second half of the experiment and could be hired to replace an original worker. We use the label "original" to describe the group of workers who participated in the first half of the experiment and the label "remaining original" to describe the group of workers who participated in the first half of the experiment and then were hired in the second half of the experiment along with a new worker.

As in the first half of each session, each firm was randomly rematched each period with workers in the second half of the experiment. However, now each firm was rematched with *three* workers each period rather than two. Two of the workers matched with the firm were "original" workers who had participated in the first half of the experiment. The third worker was a "new" worker who did not participate in the first half of the experiment. The "new" worker in the LW condition had a lower minimum wage than the two original workers, while the "new" worker in the SW condition had the same minimum wage as the two "original" workers.[8]

Amounts in the experiment were expressed in laboratory dollars. At the end of each experimental session, participants' cumulative earnings in laboratory dollars were converted to U.S. dollars.[9] Participants were then paid their earnings from the experiment, as well as their $5.00 participation fee, in cash.

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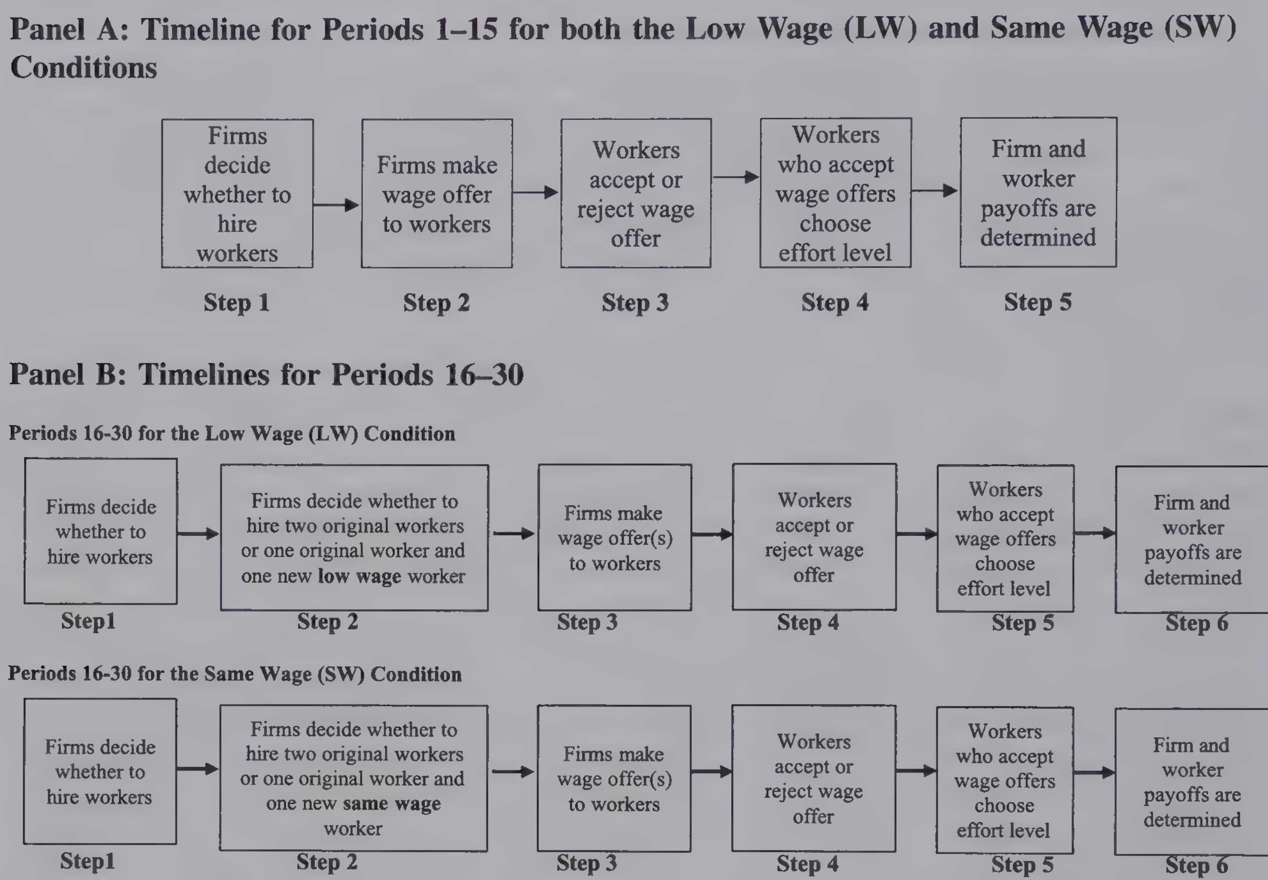
[6] We recruited our participants from the subject pool used by an experimental economics laboratory. This subject pool consists primarily of undergraduate and graduate students from a large public U.S. university, but also includes a small percentage of non-students.

[7] We randomly rematched workers and firms to be consistent with prior gift-exchange studies (Brandts and Charness 2004; Falk et al. 2006; Charness and Kuhn 2007; Gachter and Thoni 2010) and also to develop clear economic predictions for our setting. Randomly rematching workers prohibits individual reputation formation and allows clear economic predictions.

[8] Our experiment examines the effect of a change in the existing labor market (i.e., the introduction of new workers with different minimum wages) in an environment with a series of repeated anonymous interactions. As such, we follow designs used in previous economic experiments such as Fehr and Gachter (2000), Falk et al. (2006), and Owens and Kagel (2010), in which participants are randomly and anonymously rematched throughout the experiment and in which a market change is implemented during the course of the experiment.

[9] In order to keep payoffs similar for firms and workers, firms had a conversion rate of 100 laboratory dollars = $1 U.S., while workers had a conversion rate of 50 laboratory dollars = $1 U.S.

FIGURE 2
Experimental Timelines



Detailed Procedures

First Half of the Experiment

The specific steps for the first half of the experiment for both conditions are shown in Panel A of Figure 2. In Step 1, each firm was randomly matched with two workers, and firms decided whether to hire those workers. If a firm decided not to hire the workers for the period, then the period ended and both the firm and the workers received zero payoffs for that period. In Step 2, if a firm decided to hire the workers for the period, then the firm decided what wage to offer the workers. The firm made a single wage offer to both workers. Both firms and workers were explicitly informed that possible wage offers ranged from 20 to 120 in increments of 1.

In Step 3, after a firm made its wage offer, both workers learned the wage offer and decided whether to accept or reject it. If a worker rejected the wage offer, then that worker earned nothing for the period and the firm received no payoff from that worker for that period. If a worker accepted the wage offer, then the worker chose an effort level in Step 4. Workers could choose effort levels between 0.1 and 1.0. The higher the effort level a worker chose, the higher the effort cost incurred by that worker (see Table 1).

In Step 5, firms learned each of their workers' effort choices, and the firm and worker payoffs were determined. If a worker accepted an offer, then his or her payoff was determined as: *Worker payoff* = *Wage* − *Cost of effort*. The firm's payoff for each worker was determined separately as:

TABLE 1

Possible Effort Levels and Associated Costs for Workers

Effort Level	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
Cost	0	1	2	4	6	8	10	12	15	18

Firm payoff = (120 − *Wage*) × *Worker's effort level*.[10] These payoff functions, as well as the range of possible wage offers and the effort-cost schedule, are the same as those used in prior gift-exchange studies (Hannan et al. 2002; Hannan 2005; Kuang and Moser 2009, 2011).

All participants were informed of both payoff functions. In addition, at the start of the session, all participants were provided with a table that showed both the firm and worker payoff for all possible wage and effort combinations and were allowed to refer to this table throughout the session. At the end of each period, both firms and workers were shown their payoff for the period and how it was calculated, along with a history of their payoffs from all prior periods. The procedures described above were repeated in each of the first 15 periods of each session.

Second Half of the Experiment

The steps for the second half of each experimental session are shown in Panel B of Figure 2. There are two differences between the first and second halves of each session: First, participants who did not participate in the first half of the experimental session now joined the session as a third worker in the randomly formed firm-worker groups. That is, in the second half of the session, each firm was randomly rematched each period with three workers rather than two. However, as in the first half, firms could still only hire two workers. Second, the new worker matched with each firm had either a lower minimum wage of 10 (LW condition) or the same minimum wage of 20 as the two original workers (SW condition). The two original workers in both conditions continued to have the same minimum wage of 20 that they had in the first half of the session. All firms and both the new and original workers in both conditions were explicitly informed of the range of wage offers that could be made in their condition.

The detailed steps for the second half of the experiment are shown in Panel B of Figure 2 (LW condition on top, SW condition on bottom). The differences between the conditions are in Steps 2, 3, and 4. In Step 2, the firm could hire the two original workers with the same minimum wage of 20 or, alternatively, could hire one original worker and one new worker with a lower minimum wage of 10 in the LW condition or one new worker with the same minimum wage of 20 in the SW condition.[11] In Step 3, if a firm decided to hire two original workers, then the firm could make a single wage offer to the two original workers. Alternatively, if a firm decided to hire one original worker and one new worker, then the firm could make one wage offer to the new worker and a

[10] We adopted this profit function for the reasons provided in Kuang and Moser (2011). That is, we adopted it from prior studies (Fehr et al. 1997; Fehr, Kirchler, Weichbold, and Gachter 1998; Hannan et al. 2002; Hannan 2005; Kuang and Moser 2009) so that we could extend such work on reciprocity between firms and their workers. "Consistent with these earlier studies, we chose not to use the more conventional profit function (i.e., *firm profit* = 120*e* − *w*) to avoid possible negative payoffs for the firm, which are typically not allowed under institutional review board rules and could induce loss aversion (Tversky and Kahneman 1991). Prior research has shown that reciprocity between firms and employees holds under both the more conventional profit function and the profit function used in our study (Fehr et al. 1997, 1998)" (Kuang and Moser 2011).

[11] Because lower wage offers could be made to the new workers in the second half of the LW condition than could be made to workers in the first half of the condition, all participants in the LW condition were provided with an additional payoff table at the start of the second half of each experimental session that provided the firm and worker payoff for all possible wage and effort combinations, including those for the lower wage offers that could be made to the new workers.

different wage offer to one of the two original workers.[12] The specific original worker receiving the wage offer was randomly determined.

In Step 4, workers receiving a wage offer decided whether to accept or reject it. Because the firm could only hire two of the three workers, at least one of the three workers did not receive a wage offer. For example, if the firm made a wage offer to the two original workers, then the new worker did not receive a wage offer, while if the firm made one wage offer to the new worker and a second wage offer to an original worker, then the other original worker did not receive a wage offer. If either worker receiving a wage offer rejected the offer, then that worker received a zero payoff for the period. If a wage offer was rejected, then the firm was able to make a wage offer to the third worker who did not initially receive a wage offer.[13] The other steps (1, 5, and 6) are the same in both the LW and SW condition. These procedures were repeated for each period for periods 16–30 in the second half of the experiment.

V. RESULTS

Descriptive Statistics

Table 2 presents the number of wage offers made and the number of wage offers accepted by condition (Panel A), the number of wage offers accepted by worker type and by condition (Panel B), and a reconciliation of the number of the observations used in our statistical tests (Panel C). Firms almost always made wage offers (2,132 offers out of 2,160 opportunities = 98.7 percent of the time) and workers almost always accepted the firm's wage offer (2,083 acceptances out of 2,132 offers = 97.7 percent of the time). Because only 2.3 percent of firms' wage offers were rejected, we use accepted wage offers in all of our statistical tests except where noted.[14] Table 3 presents descriptive statistics for our dependent variables for the first half (Panel A) and second half (Panel B) of the experiment.

To provide a broad overview of firms' behavior, Table 4 reports the frequency with which firms made higher, lower, and equal wage offers to new workers as compared to remaining original workers in the LW and SW conditions. Consistent with our expectation that firms will make lower wage offers to new workers in the LW than in the SW condition, firms made lower wage offers to

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[12] In the LW condition, the second-half instructions informed participants that the new worker had a low minimum wage, but that the other two original workers continued to have the same high minimum wage that they had in the first half of the experiment. The instructions provided to SW condition participants did not make this distinction or use the term "minimum wage" because there was no difference between the minimum wage that could be offered to the new worker and the minimum wage that could be offered to the two original workers. This difference in wording across the two conditions could have caused the salience of the minimum-wage concept to differ across conditions and, thus, have an effect on participant behavior across conditions separate from our manipulation of a lower minimum wage for the new worker in the LW condition than in the SW condition. However, consistent with our results being driven by our manipulation and not by a difference in the salience of the minimum-wage concept across conditions, we find that firms made lower wage offers to *new* workers in the LW condition than in the SW condition, but did not make different wage offers to the *original* workers (who have the same minimum wage) across the LW and the SW conditions. Because any difference in the salience of the minimum-wage concept across the LW and SW conditions was present both when firms made wage offers to new workers and when firms made wage offers to original workers, this pattern of results suggests that participant behavior was affected by our manipulation and not by any difference in the salience of the minimum-wage concept across our conditions.

[13] There are two possibilities: (1) The initial wage offer went to the two original workers and one or both of them rejected the offer. In this case, the firm was allowed to make a new wage offer to the new worker, who could then either accept or reject it; (2) There were two separate initial wage offers, one to an original worker and the other to the new worker, and either worker rejected his or her offer (or both workers rejected their offers). In such cases, the initial wage offer to the original worker went to the second original worker, who could then either accept or reject it. As described in more detail later, very few wage offers were rejected and, thus, these possibilities almost never actually occurred.

[14] For completeness, we also conducted our statistical tests using all wage offers. The results do not change any of the statistical inferences reported in the paper.

TABLE 2

Summary Statistics: Wage Offers

Panel A: Frequency of Wage Offers, Acceptances, and Rejections by Condition

	LW Condition	SW Condition	Total
Total opportunities for wage offers	1080	1080	2160
No wage offer made	18	10	28
Initial wage offers made	1062	1070	2132
Initial wage offers rejected	33	16	49
Initial wage offers accepted	1029	1054	2083

Panel B: Frequency of Accepted Wage Offers by Condition and Worker Type

	First Half	2nd Half—New Workers	2nd Half—Remaining Original Workers	2nd Half—Both Original Workers	Total
LW Condition	505	198	203	123	1029
SW Condition	523	173	172	186	1054
Total	1028	371	375	309	2083

Panel C: Frequency of Accepted Wage Offers Used for Statistical Tests

	2nd Half—New Workers	2nd Half—Remaining Original Workers	Total
H2 and H3	371	NA	371
H4	NA	375	375
RQ—tests involving new workers	371	NA	371
RQ—tests involving remaining workers	NA	375	375

the new worker than to the remaining original worker nearly three times as often (146 times) in the LW than in the SW condition (50 times), but this pattern reverses for equal wage offers, i.e., only 39 equal wage offers in the LW condition, but 101 in the SW condition. These data provide initial evidence that firms that decide to replace an original worker with a new worker make lower wage offers to the new worker when the new worker has a lower minimum wage. Formal support for this interpretation of our results is provided in our tests of H2.

Test of H1 (Hiring of New Workers)

H1 predicts that firms will hire new workers more often in the LW condition than in the SW condition. Firms in each condition had 270 opportunities to hire a new worker in the second half of the experiment (18 firms $\times$ 15 periods in each condition).[15] Firms in the LW condition made wage

[15] We use all wage offers to test H1 because H1 is concerned with the firm's intention to hire or not hire a new worker in the second half of the experiment.

TABLE 3

Average Wage Offer, Effort, Firm Profit, Worker Payoff, and Social Welfare

Panel A: Periods 1–15

	LW Condition[a] (n = 505)	SW Condition[a] (n = 523)
Wage Offer	59	52
Effort	0.38	0.38
Firm Profit	21	24
Worker Profit	55	48
Social Welfare	76	71

Panel B: Periods 16–30

	LW Condition				SW Condition			
	Remaining Original[b] (n = 203)	New[c] (n = 198)	Both Original[d] (n = 123)	All[e] (n = 524)	Remaining Original[b] (n = 172)	New[c] (n = 173)	Both Original[d] (n = 186)	All[e] (n = 531)
Wage Offer	50	42	64	50	49	48	52	49
Effort	0.27	0.24	0.34	0.28	0.33	0.30	0.32	0.32
Firm Profit	17	17	18	17	21	20	20	20
Worker Profit	48	39	60	47	45	45	48	46
Social Welfare	65	56	78	64	66	64	68	66

[a] Data when the firm hired two original workers in the first 15 periods of the experiment.
[b] Data for the remaining original workers when the firm hired a new worker in the second half of the experiment.
[c] Data for the new workers when the firm hired a new worker in the second half of the experiment.
[d] Data for the original workers when the firm did not hire a new worker, but rather hired the two original workers in the second half of the experiment.
[e] Data for all accepted initial wage offers in the second half of the experiment.

offers to the new worker 77 percent (207/269) of the time, whereas firms in the SW condition did so 65 percent (176/270) of the time.[16]

We formally test H1 by comparing the frequency of hiring a new worker with a lower minimum wage in the LW condition to the frequency of hiring a new worker with the same minimum wage in the SW condition. We estimate a logistic regression with *New Worker* as the dependent variable, and *LW Condition* as a dichotomous independent variable.[17] *New Worker* equals 1 (0) if the firm hired (did not hire) a new worker. *LW Condition* equals 1 (0) for the LW (SW) condition. As shown for Model 1 in Panel A of Table 5, *LW Condition* is positive and marginally significant ($z = 1.44$, $p = 0.08$), providing modest support for H1.[18] That is, firms were more likely to hire new workers when this option became available in the LW condition than in the SW condition.

[16] The total number of decisions in the LW condition is 269 because, in one case, a firm in the second half of the LW condition chose not to hire any workers.
[17] To control for repeated measures, standard errors for all tests reported in the paper are estimated using Huber-White corrected standard errors clustered by participant.
[18] All reported p-values are one-tailed for directional predictions and two-tailed otherwise.

TABLE 4

Firms' Wage Offers to New Workers versus Remaining Original Workers by Condition

	LW Condition		SW Condition	
	Frequency	**Mean Difference**[a]	**Frequency**	**Mean Difference**[a]
Higher Wage Offer to New Worker[b]	22	(20.64)	25	(19.36)
Equal Wage Offers[c]	39	0	101	0
Lower Wage offer to New Worker[d]	146	16.58	50	12.28
Total	207	9.50	176	0.74

[a] Mean Difference = mean of the difference between the wage offer made to the remaining original worker and the wage offer made to the new worker.
[b] Higher Wage Offer to New Worker = cases in which the wage offer made to the new worker was higher than the wage offer made to the remaining original worker.
[c] Equal Wage Offers = cases in which the wage offer made to the new worker was the same as the wage offer made to the remaining original worker.
[d] Lower Wage Offer to New Worker = cases in which the wage offer made to the new worker was lower than the wage offer made to the remaining original worker.

Although, as reported above, we find that firms are more likely to hire a new worker when that new worker has a lower minimum wage than the same minimum wage, Figure 3 shows that that this difference decreases with experience. To examine this further, we compared firms' decisions to hire new workers in the earlier versus later periods in the second half of the experiment. We repeated the regression reported above including a dichotomous variable for *LaterPeriod* (equal to 1 [0] if the observation is from the last eight periods [first seven periods]) and the interaction variable *LaterPeriod* × *LW Condition*. Consistent with the pattern in Figure 3, the results for Model 2 in Panel A of Table 5 yield a significant *LaterPeriod* × *LW Condition* interaction ($z = -2.19$, $p = 0.03$).[19] Thus, the tendency to hire new workers more frequently in the LW condition than in the SW condition was less pronounced in the later periods.

Test of H2 (New Worker Wages)

H2 predicts that firms will make lower wage offers to new lower-wage workers in the LW condition than to new same-wage workers in the SW condition. We test H2 using a random-effects regression with *NW Wage* as the dependent variable and *LW Condition* and *First-Half Wage* as independent variables.[20] *NW Wage* is equal to the wage offer made to the new worker in the second half of the experiment. *LW Condition* is defined as described for our tests of H1. *First-Half Wage* is the average wage paid by each firm in the first half of the experiment. We control for *First-Half*

[19] Because this result provides evidence of a difference in behavior over time between the LW and SW conditions, we conducted all tests reported in the paper controlling for the effect of period both as an ordinal variable using each period and also as a dichotomous variable by splitting periods into an early period group (periods 16–22) and a late period group (periods 23–30). Because controlling for period did not change any of our statistical inferences, we do not include period in the regression models reported in the paper.

[20] Consistent with prior research (Brandts and Charness 2004; Gachter and Thoni 2010; Owens and Kagel 2010), we use Generalized Least Squares (GLS) random-effects models in this test, as well as in all subsequent tests, because the variation across participants is assumed to be random and uncorrelated with the independent variables in our models. Our results are unchanged when we instead conduct Ordinary Least Squares (OLS) regressions.

TABLE 5

Tests of H1 and H2

Panel A: Tests of H1

	Coefficient	z-value	p-value[h]
Model 1: *New Worker*[a] $= \alpha_1 + \alpha_2$*LW Condition* $+ \varepsilon$.			
Constant	0.63	2.44	0.02
LW Condition[b]	0.58	1.44	0.08[c]
Wald Chi-square	2.07		
Number of observations	539		
Model 2: *New Worker*[a] $= \alpha_1 + \alpha_2$*LW Condition* $+ \alpha_3$*Period* $+ \alpha_4$*Period* $\times$ *LW Condition* $+ \varepsilon$.			
Constant	0.74	2.68	<0.01
LW Condition[b]	1.17	2.57	0.01
LaterPeriod[d]	−0.18	−0.86	0.39
LaterPeriod $\times$ *LW Condition*[e]	−0.87	−2.19	0.03
Wald Chi-square	13.24		
Number of observations	539		

Panel B: Test of H2

	Coefficient	z-value	p-value[h]
Model: *NW Wage*[f] $= \alpha_1 + \alpha_2$*LW Condition* $+ \alpha_3$*First-Half Wage* $+ \varepsilon$.			
Constant	4.06	0.51	0.61
LW Condition[b]	−10.66	−2.92	<0.01
First-Half Wage[g]	0.85	6.21	<0.01
Wald Chi-square	46.09		
Number of observations	371		

[a] *New Worker* is an indicator variable that equals 1 (0) if the firm chooses to hire (not to hire) a new worker.
[b] *LW Condition* is an indicator variable that equals 1 (0) if the firm is in the LW (SW) condition.
[c] One-tailed p-value based on the directional prediction for this variable; all other p-values presented are two-tailed p-values.
[d] *LaterPeriod* is an indicator variable that equals 1 if the observation is in the last eight periods of the second half of the experiment.
[e] *LaterPeriod* $\times$ *LW Condition* is the interaction between the *LaterPeriod* and *LW Condition* variables.
[f] *NW Wage* is the wage offer made to the new worker in the second half of the experiment.
[g] *First-Half Wage* is equal to the average wages paid by each firm in the first half of the experiment.
[h] p-values are estimated using Huber-White corrected standard errors clustered by participant.

Wage in this regression because we expect a positive relation between a firm's wage offers in the first and second halves of the experiment.

Consistent with H2, the results of the regression reported in Panel B of Table 5 show that *LW Condition* is negative and significant ($z = -2.92$, $p < 0.01$), indicating that firms made lower wage offers to new workers in the LW condition than in the SW condition. This result is consistent with firms expecting that the new workers will use their lower minimum wage as a reference point when evaluating the size of the gift in the firm's wage offer and, therefore, trusting the new workers to reciprocate the firm's lower wage offer with effort similar to that provided by the higher-wage workers for a higher wage. In addition to supporting H2, the results reported in Panel B of Table 5

FIGURE 3
Firms' Decisions to Hire New Workers

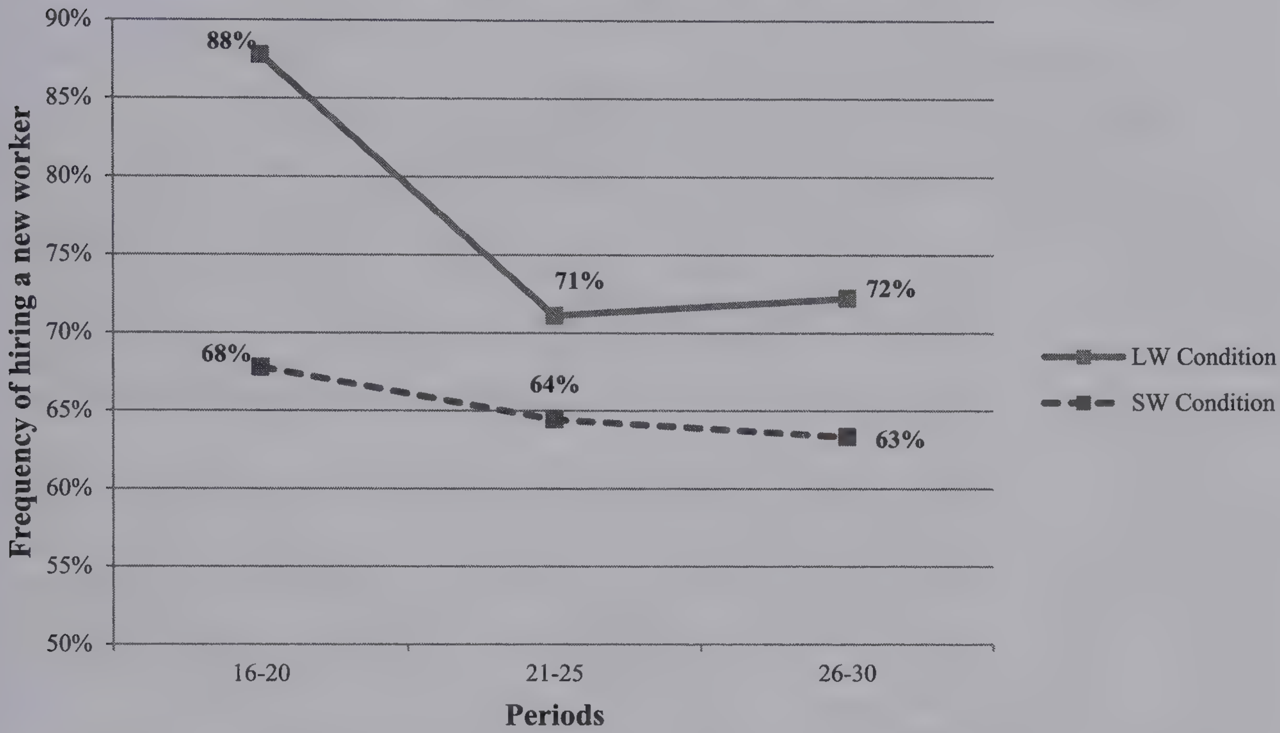

Figure 3 presents firms' decisions to hire new workers in the LW and SW conditions during the second half of the experiment. The percent of time firms hired new workers is calculated as the number of times firms chose to hire new workers divided by the total number of hiring decisions made by firms. Overall, in the LW condition, firms chose to hire new workers 77 percent (207/269) of the time and firms in the SW condition chose to hire new workers 65 percent (176/270) of the time.

show that, as expected, *First-Half Wage* is a significant predictor of the wages offered to new workers in the second half of the experiment.

Although, as reported above, firms in the LW condition hired new workers and paid them a lower wage when this option was available, it appears that this option was more attractive to certain types of firms. Most firms engaged in gift exchange in the first 15 periods, as evidenced by the fact that very few firms offered the lowest wage possible. However, some firms offered wages well above the minimum wage (i.e., the more-trusting firms), while other firms offered lower wages (i.e., the less-trusting firms). LW-condition firms that offered lower wages in the first half of the experiment were more likely to hire new lower-wage workers in the second half of the experiment. Specifically, although the overall average wage offer made by LW-condition firms in the first half of the experiment is 59, the average first-half wage offers of LW-condition firms that hired new workers more frequently in the second half of the experiment is only 54, whereas the average first-half wage offers of LW-condition firms that hired new lower-wage workers less frequently in the second half of the experiment is 63.[21] Thus, hiring new workers with a lower minimum wage in the LW condition was apparently more attractive to firms that made lower wage offers in the first half

[21] Firms who hired new lower-wage workers more than two-thirds of the time were classified as doing so more frequently. The wage offers of firms who hired new lower-wage workers more frequently (54) are significantly lower ($t = 4.13$, $p < 0.01$) than the wage offers of firms who hired new lower-wage workers less frequently (63).

of the experiment.[22] Because new workers in the SW condition had the same minimum wage in the second half of the experiment as the original workers, this pattern of behavior did not occur in the SW condition.

Test of H3 (New Worker Effort)

H3 predicts that new lower-wage workers will provide greater effort for a given wage than new same-wage workers because new lower-wage workers will perceive their wage offers to include larger gifts. New workers in the LW condition could view a given wage offer as including a larger gift because they have a lower minimum wage than new workers in the SW condition. Therefore, the difference in minimum wages across the two conditions captures whether new workers evaluate their wage offers relative to their minimum wage. Accordingly, we test H3 by comparing new workers' effort responses in the LW condition to that of new workers in the SW condition using a regression with the new workers' effort, *NW Effort*, as the dependent variable, and *LW Condition* as our primary independent variable.

LW Condition is the same dichotomous variable for condition as used in our tests of H1 and H2. *NW Wage* and *Wage Diff* are included in the regression as control variables. *NW Wage* is the new worker's wage, and *Wage Diff* is the difference between the new worker's wage and the remaining original worker's wage. We control for new workers' wages because prior gift-exchange studies document a positive association between the wages and effort (e.g., Fehr et al. 1993, 1996) and because this also controls for any possible differential firm-level effects across conditions. We control for any difference between new workers' wages and the remaining original workers' wages because such wage differences have been shown to affect worker effort in some settings. Controlling for these variables allows us to isolate any effect of a lower minimum wage beyond the normal gift-exchange response and any response to wage differences between the new and remaining original workers.

The results reported for Model 1 in Panel A of Table 6 indicate that our primary variable of interest, *LW Condition,* is not significant ($z = -0.33$, $p = 0.37$), suggesting that, inconsistent with H3, new workers did not evaluate the size of the gift in their wage based on their lower minimum wage. Moreover, *NW Wage* is significantly related to effort ($z = 5.18$, $p < 0.01$), which is the typical gift-exchange result. Thus, new workers in both the LW and SW conditions appear to have based their effort only on their absolute wage level, lowering their effort in response to their lower wages. Finally, *Wage Diff* is not significant ($z = 0.46$, $p = 0.64$).[23]

Although the analysis reported above shows that, on average, new workers did not evaluate the size of the gift in their wage relative to their lower minimum wage, that analysis cannot tell us whether the lower minimum wage in the LW condition had a different effect on new worker effort at different levels of wages. To test this, we repeat the regression reported above including the interaction term *NW Wage* × *LW Condition.* The results are reported for Model 2 in Panel A of Table 6. The interaction term is marginally significant ($z = -1.74$, $p = 0.08$), providing modest evidence that the impact of condition on new worker effort depends on the wage level. Panel A of Figure 4 depicts

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[22] Because this result shows that different types of firms may be self-selecting into hiring new workers across the LW and SW conditions, it is important to control for this possibility in any tests for which the dependent variable could be affected by such self-selection. We control for this possibility in our tests of H2 and our research question by including a control variable for the average first-half value of the dependent variable at the individual firm level. For example, since the dependent variable in our tests of H2 was the wage offer made to the new worker in the second half of the experiment, we included a control variable for average first-half wage offers made by the firm in our tests of H2. We also control for any possible effects of firm self-selection in our tests of worker effort in H3 and H4 by controlling for the wage offer made by the firm in the second half of the experiment.

[23] This result is consistent with the findings of Charness and Kuhn (2007) and suggests that the new workers in the LW condition believed that their lower wage offers were justified by their lower minimum wage.

TABLE 6

Tests of H3 and H4

Panel A: Tests of H3

	Coefficient	z-value	p-value[k]
Model 1: $NW\ Effort^{a} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 NW\ Wage + \alpha_4 Wage\ Diff + \varepsilon$.			
Constant	0.05	0.95	0.34
LW Condition[b]	−0.02	−0.33	0.37[c]
NW Wage[d]	0.01	5.18	<0.01
Wage Diff[e]	0.0003	0.46	0.64
Wald Chi-square	30.56		
Number of observations	371		
Model 2: $NW\ Effort^{a} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 NW\ Wage + \alpha_4 LW\ Condition \times NW\ Wage + \alpha_5 Wage\ Diff + \varepsilon$.			
Constant	−0.07	−1.30	0.19
LW Condition[b]	0.14	2.36	0.02
NW Wage[d]	0.01	5.05	<0.01
NW Wage × LW Condition[f]	−0.003	−1.74	0.08
Wage Diff[e]	0.0003	0.44	0.66
Wald Chi-square	39.95		
Number of observations	371		

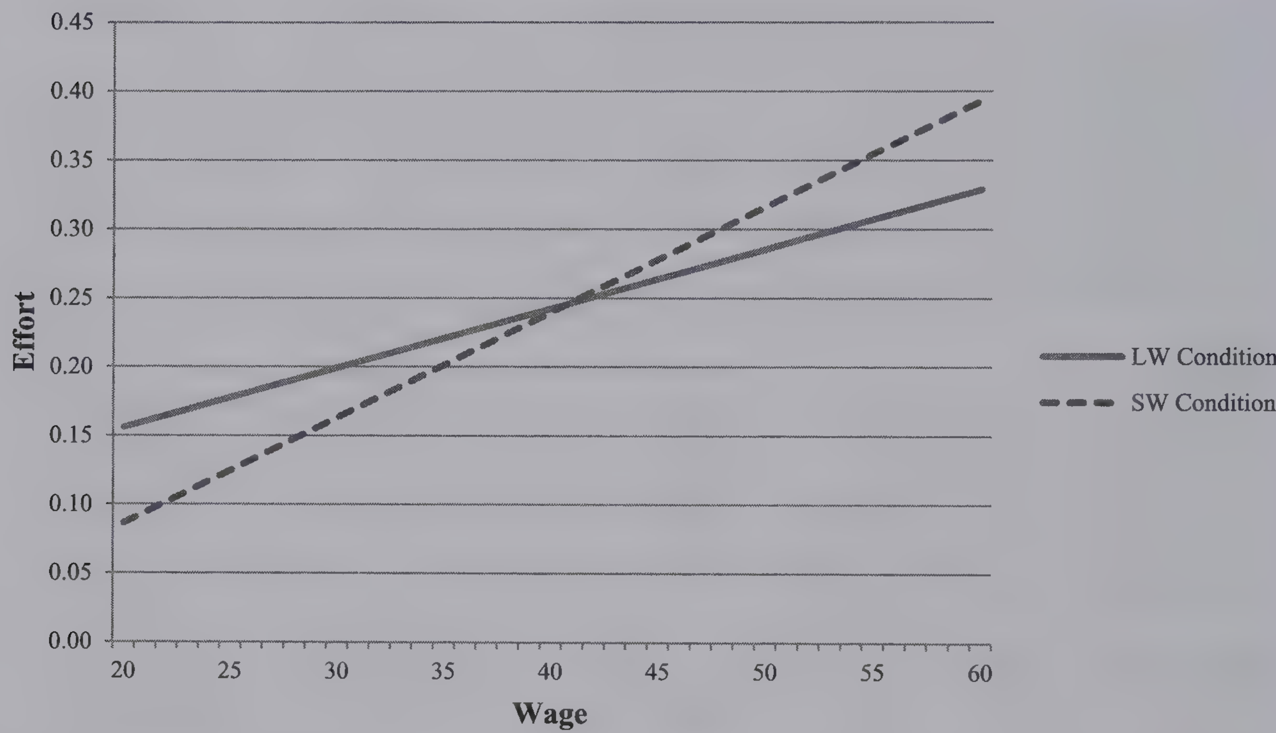
Panel B: Tests of H4

	Coefficient	z-value	p-value[k]
Model 1: $RW\ Effort^{g} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 RW\ Wage + \alpha_4 Wage\ Diff + \varepsilon$.			
Constant	0.07	1.58	0.11
LW Condition[b]	−0.07	−1.64	0.05[c]
RW Wage[h]	0.01	6.28	<0.01
Wage Diff[i]	0.001	1.00	0.32
Wald Chi-square	49.80		
Number of observations	375		
Model 2: $RW\ Effort^{g} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 RW\ Wage + \alpha_4 LW\ Condition \times RW\ Wage + \alpha_5 Wage\ Diff + \varepsilon$.			
Constant	−0.03	−0.51	0.61
LW Condition[b]	0.07	1.12	0.26
RW Wage[h]	0.01	5.54	<0.01
RW Wage × LW Condition[j]	−0.003	−1.67	0.10
Wage Diff[i]	0.0003	0.62	0.54
Wald Chi-square	54.25		
Number of observations	375		

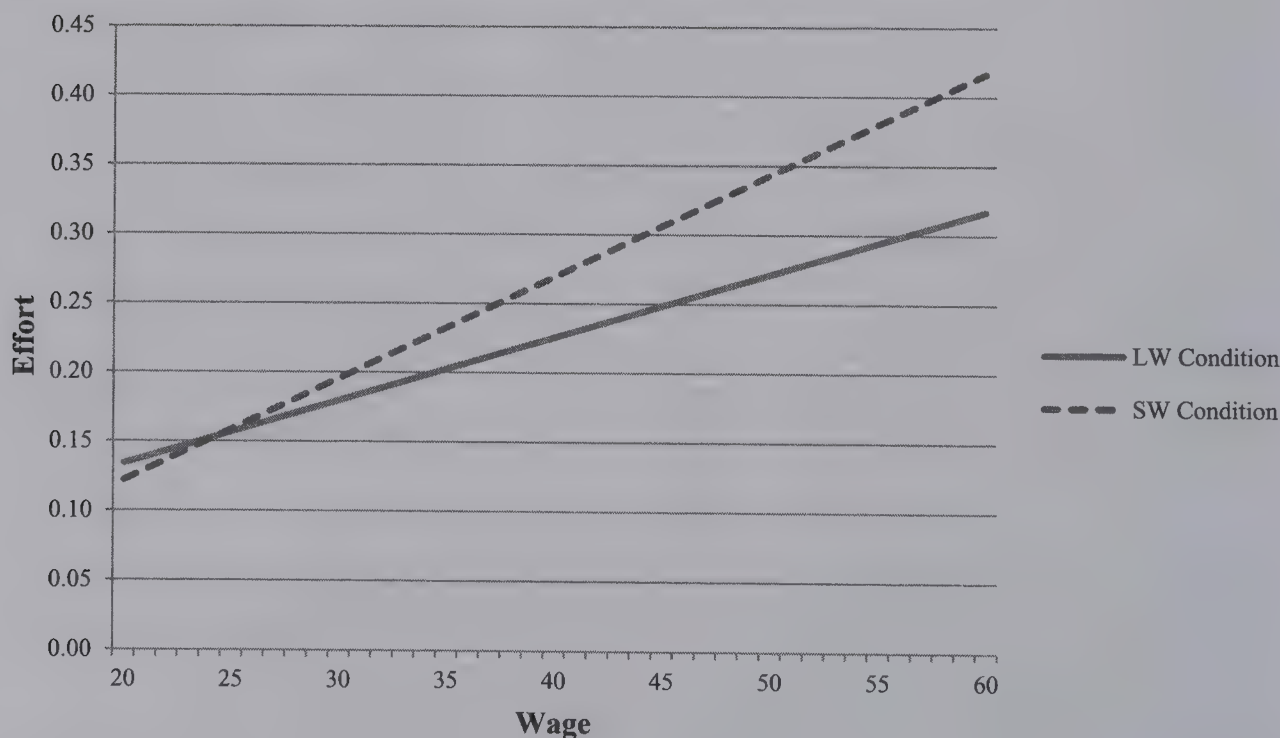
[a] *NW Effort* is the new worker's effort level when the firm hires a new worker.
[b] *LW Condition* equals 1 (0) if the observation is in the LW (SW) condition.
[c] One-tailed p-value based on the directional prediction for this variable; all other p-values presented are two-tailed p-values.
[d] *NW Wage* is equal to the wage offer made to the new worker when the firm hires a new worker.
[e] *Wage Diff* is equal to the wage offer made to the remaining worker minus the wage offer made to the new worker.
[f] *NW Wage × LW Condition* is the interaction between the *LW Condition* and *NW Wage* variables.
[g] *RW Effort* is the remaining worker's effort level when the firm hires a new worker.
[h] *RW Wage* is equal to the wage offer made to the remaining worker when the firm hires a new worker.
[i] *Wage Diff* is equal to the wage offer made to the new worker minus the wage offer made to the remaining worker.
[j] *RW Wage × LW Condition* is the interaction between the *LW Condition* and *RW Wage* variables.
[k] p-values are estimated using Huber-White corrected standard errors clustered by participant.

FIGURE 4
Wage-Effort Relationship

Panel A: New Worker



Panel B: Remaining Worker



the relationship and shows that for wage offers of less than 40, the results are consistent with H3 in that new workers provide more effort for a given wage in the LW condition than in the SW condition. However, for wage offers above 40, workers provide more effort for a given wage in the SW condition than in the LW condition, which is opposite to the relation predicted in H3. While we did not predict this pattern of results, it is consistent with results reported by Owens and Kagel (2010), who only found a positive effect of minimum wages on effort when wages were closer to the minimum wage.

Test of H4 (Remaining Original Worker Effort)

H4 predicts that the remaining original workers will respond with lower effort when firms hire a new lower-wage worker than when firms hire a new same-wage worker. We test H4 by examining remaining workers' effort in the LW condition versus the SW condition. Specifically, we estimate a regression with the remaining workers' effort, *RW Effort,* as the dependent variable and *RW Wage, Wage Diff,* and *LW Condition* as independent variables. *LW Condition* is the main variable of interest and is the same dichotomous variable for condition used in our earlier tests. Similar to the regression used to test H3, we control for *RW Wage* and *Wage Diff* in order to isolate any possible effect of *LW Condition* on the remaining workers' effort beyond the effect of wages and differences in wages between the remaining workers and the new workers. *RW Wage* is the remaining original workers' wage and *Wage Diff* is defined as for H3.

The results, which are reported for Model 1 in Panel B of Table 6, show that, consistent with H4, *LW Condition* is significant ($z = -1.64$, $p = 0.05$), indicating that the remaining workers in the LW condition responded with lower effort than the remaining workers in the SW condition. As expected, *RW Wage* also significantly affects effort ($z = 6.28$, $p < 0.01$), which is the typical gift-exchange result. *Wage Diff* is not significantly related to effort ($z = 1.00$, $p = 0.32$), indicating that the remaining workers' effort choices were not significantly influenced by any differences between their wages and the wages of the new workers.

The results reported above describe average remaining original worker behavior for all wage levels, but it is possible that the difference between remaining original workers' effort in the LW and SW conditions varied by wage level. To examine this issue, we repeat the regression reported above including the interaction term *RW Wage* × *LW Condition*. The results of this regression are reported for Model 2 in Panel B of Table 6. The interaction term is marginally significant ($z = -1.67$, $p = 0.10$), providing modest evidence that the impact of condition on remaining worker effort depends on the wage level. Panel B of Figure 4 depicts the relationship between wages and effort for remaining workers in the two conditions. As shown in Figure 4, the remaining workers' negative reaction in the LW condition to the firm's decision to hire a new worker increases as the wage level increases. In summary, Model 1 in Panel B of Table 6 provides support for H4, and Model 2 in Panel B of Table 6 and Panel B of Figure 4 show that this support increases for higher wages.

Tests of Research Question (Firm Profit, Worker Payoff, and Social Welfare)

Our research question relates to the effect of hiring a new lower-wage worker versus a new same-wage worker on firm profit, worker payoffs, and social welfare. When the firm hires a new worker, it also hires a remaining original worker. Thus, the impact of hiring a new lower-wage worker versus a new same-wage worker on firm profit and workers' payoffs depends on the impact on both the new and remaining original workers. Because the impact differs for the new and remaining original workers, we first examine the new and remaining original workers separately in our analyses.

TABLE 7

Tests of Research Question

Panel A: Tests of Firm Profit

	Coefficient	z-value	p-value[h]
Model 1: $NW\ Profit^{a} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 First\text{-}Half\ Profit + \varepsilon$.			
Constant	4.95	1.55	0.12
LW Condition[b]	−1.47	−0.88	0.38
First-Half Profit[c]	0.63	4.85	<0.01
Wald Chi-square	32.73		
Number of observations	371		
Model 2: $RW\ Profit^{d} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 First\text{-}Half\ Profit + \varepsilon$.			
Constant	6.10	1.57	0.12
LW Condition[b]	−2.34	−1.55	0.12
First-Half Profit[c]	0.65	4.28	<0.01
Wald Chi-square	25.36		
Number of observations	375		

Panel B: Tests of Worker Payoffs

	Coefficient	z-value	p-value[h]
Model 3: $NW\ Payoff^{e} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 First\text{-}Half\ Payoff + \varepsilon$.			
Constant	2.79	0.35	0.73
LW Condition[b]	−10.43	−3.07	<0.01
First Half Payoff[f]	0.89	5.92	<0.01
Wald Chi-square	42.27		
Number of observations	371		
Model 4: $RW\ Payoff^{g} = \alpha_1 + \alpha_2 LW\ Condition + \alpha_3 First\text{-}Half\ Payoff + \varepsilon$.			
Constant	1.24	0.20	0.85
LW Condition[b]	−2.24	−0.84	0.40
First-Half Payoff[f]	0.92	7.76	<0.01
Wald Chi-square	60.43		
Number of observations	375		

[a] *NW Profit* is equal to the firm's profit from hiring a new worker in the second half of the experiment.
[b] *LW Condition* equals 1 (0) if the firm is in the LW (SW) condition.
[c] *First-Half Profit* is equal to the average profit per worker for each firm in the first half of the experiment.
[d] *RW Profit* is equal to the firm's profit from the remaining worker when hiring a new worker.
[e] *NW Payoff* is equal to the new workers' payoff when they accept an initial wage offer made to them by the firm.
[f] *First-Half Payoff* is equal to each firm's average worker payoff in the first half of the experiment.
[g] *RW Payoff* is equal to the remaining workers' payoff when they accept an initial wage offer made to them by the firm.
[h] p-values are estimated using Huber-White corrected standard errors clustered by participant.

Firm Profit

Model 1 in Panel A of Table 7 presents the results of a regression that compares the firm profit from new workers in the LW condition to firm profit from new workers in the SW condition, while controlling for first-half firm profit. The dependent variable is *NW Profit,* which is the amount of profit the firm earns from each new worker. The two independent variables are *LW Condition* (as defined for our previous tests) and *First-Half Profit* (the average first-half profit for each firm). We

include *First-Half Profit* to control for any differences in firm profits across the LW and SW conditions in the first half of the experiment. Because *LW Condition* measures whether firm profit from new workers differs between the LW and SW conditions, this is the main variable of interest. As shown for Model 1 in Panel A of Table 7, *LW Condition* is not significant ($z = -0.88$, $p = 0.38$), indicating that hiring a new lower-wage worker does not increase the firm's profit from the new worker. Thus, even though firms hired new workers more often and paid them lower wages in the LW condition than in the SW condition, firm profit was not higher in the LW condition than in the SW condition.

We next examine the effect on firm profit from the remaining original worker using the same regression as in Model 1, except that the dependent variable for this analysis is firm profit from the remaining worker (*RW Profit*) rather than *NW Profit*. As shown for Model 2 in Panel A of Table 7, *LW Condition* is marginally significant ($z = -1.55$, $p = 0.12$), indicating that firm profit from the remaining original worker is marginally *lower* when the firm hires a new lower-wage worker.[24] This result is consistent with our earlier tests of H4 that found that the remaining workers reduced their effort when firms hired a new lower-wage worker. Finally, we test the combined effect on firm profit of both new and remaining original workers and find similar results for firm profit between the LW and SW conditions ($p = 0.14$, untabulated) when the firm hires a new worker.

Worker Payoff

Models 3 and 4 in Panel B of Table 7 report the results of regressions that compare worker payoffs when a firm hires a new worker in the LW versus SW condition for the new and remaining original workers, after controlling for first-half worker payoffs. The dependent variables are *NW Payoff* (the new workers' net earnings) in Model 3 and *RW Payoff* (the remaining original workers' net earnings) in Model 4. We include *First-Half Payoff* (the average payoff for each worker in the first half of the experiment) in both models to control for any differences in worker payoff in the first half of the experiment. The variable of interest in both models is *LW Condition* (as defined in previous tests). The results for Model 3 in Panel B of Table 7 show that the new workers' payoffs are lower in the LW condition than in the SW condition ($z = -3.07$, $p < 0.01$), reflecting the fact that the combination of the lower wages they received and the effort they provided resulted in lower payoffs. The results for Model 4 in Panel B of Table 7 show that the remaining original workers' payoffs are not different across the LW and SW conditions ($z = -0.84$, $p = 0.40$). Because new workers' payoffs are lower, the combined payoff for the new and remaining original workers is also significantly lower ($z = -2.28$, $p = 0.02$, untabulated) in the LW condition than in the SW condition when the firm hires a new worker.

With no improvement in firm profit, no difference in the remaining workers' payoffs, and a significant decline in the new lower-wage workers' payoffs, a formal statistical test of social welfare (not tabulated) shows that, as expected, social welfare is lower ($z = -3.71$, $p < 0.01$) in the LW condition than in the SW condition.[25]

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[24] We report a two-tailed p-value because this is a test of a research question in which we did not have an *a priori* directional prediction. However, a one-tailed p-value would also be appropriate because, given that we know from our test of H4 that the remaining workers' effort decreased, the logical direction of the effect on firm profit could only be a decrease.

[25] We also examine the effects when firms chose not to hire new lower-wage workers when the option was available and find that there are no significant differences in wage offers, workers' effort, firm profit, workers' payoffs, or social welfare across the LW and SW conditions (all p-values > 0.23). These results increase our confidence that, as hypothesized, the differences across conditions that we document in our main analyses are driven by the firms' decisions to replace an original worker with a new lower-wage worker.

VI. CONCLUSION

As firms often do in practice, firms in our experiment replaced existing higher-wage workers with new lower-wage workers when this option became available. However, despite paying these new workers lower wages, firm profit did not increase for two reasons. First, the new lower-wage workers provided lower effort in response to lower wage offers and, second, the remaining original workers lowered their effort in response to the firm's decision to replace existing workers with new lower-wage workers. Finally, the decision to hire new lower-wage workers decreased new workers' payoffs and, therefore, social welfare also decreased.

Our results extend prior studies in accounting and economics that investigate the impact of social norms on firm and worker behavior (Fehr et al. 1993; Hannan et al. 2002; Towry 2003; Hannan 2005; Zhang 2008; Kuang and Moser 2009, 2011; Tayler and Bloomfield 2011; Christ et al. 2012). Specifically, we identify unintended costs of replacing existing workers with new lower-wage workers that neither management accountants, who assess the financial feasibility of this strategy, nor firm managers, who ultimately decide whether to adopt this strategy, may anticipate. Anticipating the full effect of replacing existing workers with new lower-wage workers on firm profit is critical because firm managers may not adopt this strategy if they are unsure about the effect on firm profit. Moreover, if hiring new lower-wage workers does not increase firm profit and lowers workers' payoffs, then social welfare declines. Consequently, even if hiring new lower-wage workers is expected to increase firm profit, some firm managers may decide not to take this action because of the potential negative effect on social welfare. Finally, even if firm managers hire new lower-wage workers to increase firm profit despite the potential negative effect on social welfare, government officials may want to establish policies to discourage such behavior.

Although we believe that our setting captures critical aspects of actual field settings in which firms have the option to hire new lower-wage workers, there are, of course, aspects of specific field settings that are not captured in our experiment. For example, we do not examine whether the effects of hiring new lower-wage workers vary depending on any national, ethnic, or cultural differences between the remaining original workers and the new lower-wage workers. Remaining original workers could react more negatively than in our experiment if the new lower-wage workers are from a different country or different ethnic or cultural background, and this could lead to even lower firm profit and social welfare than observed in our experiment. However, it is also possible that new workers in a more distant location would be more aware of, and focused on, the gift in their wage offers than the new workers in our study and, thus, provide more effort than the new workers in our study. Future research could investigate whether and how such ethnic, cultural, or location differences might affect our findings.

Another potential limitation of our study is that we did not allow individual reputation formation via repeated interaction between the same firms and workers. However, we note that allowing such individual reputation formation over multiple periods would not likely affect our results because all of our statistical tests compare outcomes in the LW condition versus the SW condition. Because any such individual reputation effects would likely be similar across these conditions, any differences across the conditions that we identified in our study would likely be similar in a setting with individual reputation. Nevertheless, because our design prevented such individual reputation formation, we cannot be certain whether allowing it would affect our results. Future research could examine whether and how allowing such individual reputation formation would affect firm and worker behavior when the firm can replace a worker with a new lower-wage worker.

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Accounting Information Quality, Interbank Competition, and Bank Risk-Taking

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ABSTRACT: We study the interaction between interbank competition and accounting information quality and their effects on banks' risk-taking behavior. We identify an endogenous false-alarm cost that banks incur when forced to sell assets to meet capital requirements. We find that when the interbank competition is less intense, an improvement in the quality of accounting information encourages banks to take more risk. Keeping the banks' investments in loans constant, the provision of high-quality accounting information reduces the false-alarm cost of assets sales and improves the discriminating efficiency of the capital requirement policy. When considering the banks' endogenous investment decisions, however, this improvement in discriminating efficiency causes excessive risk-taking, because banks respond by competing more aggressively in the deposit market, and the increase in deposit costs motivates banks to take more risk. Our paper shows that improving information quality increases risk-taking with mild competition, but has no effect under fierce competition.

Keywords: *information quality, bank risk-taking, interbank competition.*

I. INTRODUCTION

Limited liability in the form of, for instance, deposit insurance provides an incentive for financial institutions to increase risk via both capital structure and investment decisions. Not surprisingly, concerns about such risk-taking are paramount for those who study, devise, and implement regulations for the financial system. Because competition within the

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financial system is not perfect, one avenue of inquiry regarding risk has been to explore how the extent of competition influences bank risk-taking when the limited liability incentives are present. Within the context of this exploration, some regulators and academics argue that to preserve the stability of the banking and financial industry, competition needs to be restrained (Padoa-Schioppa 2001, 14; Keeley 1990; Suarez 1994; Matutes and Vives 1996). We extend this literature regarding the relation between bank risk-taking and competition by exploring how two other risk disciplining mechanisms alter the behavior of banks when competition ranges from near-monopoly to perfect competition: minimum capital requirements and accounting disclosure quality.

Within the context of an imperfect competition model, we show that the presence of an accounting-based capital requirement, which is intended to reduce risks associated with high leverage, creates an important role for accounting information quality in bank risk-taking via the opportunity costs of false negative signals from the accounting system. Most importantly, we show that the impact of disclosure quality on risk-taking depends directly on the level of competition in the banking market. Specifically, we find that when the competition in the deposit market is less intense, risk-taking incentives are increasing in the quality of disclosure. However, this positive relation between disclosure quality and risk-taking becomes more muted as competition increases such that when the competition is sufficiently fierce, disclosure quality has no impact on risk-taking incentives. We also show that requiring banks to hold a minimum amount of capital restrains their risk-taking behavior. However, this disciplining effect is weakened when information quality is increased. Thus, while the banking literature posits capital requirements, competition policy, and disclosure as key policy tools to discipline bank risk-taking, we show that these policies cannot be examined in isolation because the interaction among them plays a key role in determining risk-taking incentives.

We examine a setting in which N banks compete in the deposit market. Each bank chooses the amount of capital to raise through deposits and the level of risk at which this capital is invested in loans. After banks take these decisions simultaneously, a public accounting signal issued by each bank provides information about the quality of its loan investment. This accounting information is used by a regulator to monitor whether banks meet a regulatory capital requirement. If a bank fails to meet the requirement, then it is forced to sell a portion of its risky assets to boost its capital ratio. This setting allows us to examine the interaction between the two banking regulatory tools of capital requirements and accounting information. Because the capital-ratio requirement is calculated based on accounting information, the ability of a capital requirement policy to deter banks' risk-taking behavior should be examined jointly with the financial accounting information properties. We examine this interaction assuming that banks improve their capital ratio through the sale of a portion of their risky assets. This is a frequently observed action taken by banks to fulfill the capital requirement. During the 2008–2009 financial crisis, following huge write-downs and severe capital impairments, banks were often forced to deleverage by selling a considerable amount of their risky assets in the secondary market, even at a distressed or fire-sale price (Shleifer and Vishny 2011). For instance, First Financial Network, an Oklahoma City-based loan sale advisor on behalf of the Federal Deposit Insurance Corporation (FDIC), planned to sell \$150 million in loan participation from four failed banks in October 2009 (*Business Wire* 2009). More recently, BNP Paribas, one of the largest French banks, sold \$96 billion of assets to shore up capital and cut funding needs (Reuters 2011).

The results in our article stem mainly from the emergence of an endogenous "false-alarm" cost that banks incur when they are forced to sell their risky assets, even if they are sold at their fair price. This cost is borne only by a bank that receives a bad accounting signal, but ultimately remains solvent. The false-alarm cost arises because when the bank sells its assets, neither the bank nor the market knows the future outcome. Consequently, the fair price that the market offers reflects the expected cash flows considering the possibility of both good and bad outcomes. In the case that a bad investment outcome is realized, the bank is insolvent and must use all of the proceeds to repay

depositors. Since insolvency happens regardless of whether assets are sold, asset sales have no net effect on the final payoff, which is always zero. However, if a good investment outcome is realized, then the cash flow the assets yield is larger than the proceeds obtained from their sale. Therefore, the "early" assets sales triggered by a false alarm from the imperfect accounting information system impose a cost *ex ante*.

We show that the false-alarm cost of assets sales plays an important role in the relation between accounting information quality and banks' risk-taking decisions. When the number of banks competing in the same market is not too large, we find that an improvement in accounting information quality induces more aggressive risk-taking. More specifically, holding the amount of banks' investments in loans constant, the provision of high-quality accounting information reduces the false-alarm cost of assets sales and improves the discriminating efficiency of the capital requirement policy, which is consistent with conventional wisdom. However, if the banks' investment decisions are optimally determined, then it is precisely this improvement in discriminating efficiency that may cause excessive risk-taking, because the reduced endogenous false-alarm cost implies higher investment returns. As a result, banks respond by expanding investments, which, through competition in the deposits market, leads to a higher deposit rate. The higher deposit rate, in turn, lowers the banks' profitability, which induces them to take riskier loan investments to maintain their level of profitability. In contrast, when there are a sufficiently large number of banks, we find that accounting information quality has no impact on banks' risk-taking decisions. Upon a bad signal, a bank's profit is outweighed by the false-alarm cost, and that results in the bank's insolvency even if the bank's investment yields a high outcome. As a result, the bank only cares about its payoff after a good signal, which is not affected by accounting information quality. This response makes the risk decision independent from information quality.

We also study an extension of our main setting that examines the effect of accounting conservatism on banks' risk-taking decisions. We find that a more conservative accounting system restrains banks' risk-taking behavior when the interbank competition is less intense. In contrast, when there are many banks competing, neither information quality nor conservatism influences banks' risk-taking decisions. Indeed, in our setting, accounting information is only relevant through the capital requirement examination. Therefore, the quality of information only plays a role in the case of a bad signal. Because conservatism makes bad signals less informative, increasing conservatism in our setting is equivalent to decreasing information quality.

Section II next provides a literature review. We describe the main model in Section III, and explain the resulting equilibrium in Section IV. Section V provides an extension of our model to study the effects of accounting conservatism. Section VI provides several robustness checks to our main results and discusses caveats. Section VII concludes.

II. LITERATURE REVIEW

Prior literature has extensively examined the interaction between market competition and risk-taking behavior in the banking industry. Some studies argue that a less competitive environment allows banks to enjoy higher rents that they would lose in case of failure. Therefore, lowering competition might improve economic efficiency by inducing banks to be more cautious in their risk-taking behavior to avoid failure (Allen and Gale 2000; Keeley 1990; Suarez 1994; Matutes and Vives 1996). This argument is also shared by some banking regulators. However, other studies reach different conclusions. For example, Boyd and Nicolò (2005) study a setting in which a bank offers a menu of contracts to borrowers, and they argue that banks can be more aggressive in risk-taking as the market becomes more concentrated. The related empirical evidence on this matter is mixed. Some studies show that bank crises are less common in more concentrated markets (Beck, Demirgüç-Kunt, and Levine 2003; Keeley 1990; Dick 2006), while some other studies reach the

opposite conclusion (Jayaratne and Strahan 1998). In the extant literature, the role of accounting disclosure on the interaction between market competition and risk-taking has been ignored. In our article, we shed light on this interaction by assuming that banks, after taking their investment size and risk decisions, are subject to a capital requirement examination that uses accounting information. We find that both harsher competition and more precise information increase risk, although the effect of information quality on risk vanishes when competition is too harsh.

A second stream of related literature examines the relationship between capital requirements and banks' risk-taking behavior. Buser, Chen, and Kane (1981) provide insight on how raising capital requirements may restrict banks' risk-taking behavior. Regulators apparently share this point of view and believe that a tightened capital requirement is an effective way to restrain aggressive leverage-taking. However, there are also studies indicating that the effect of a capital requirement on a bank's risk-taking behavior is not monotonic (Koehn and Santomero 1980; Gennotte and Pyle 1991). In addition, empirical studies on the relation between capital requirements and banks' risk-taking provide mixed evidence (Aggarwal and Jacques 2001; Konishi and Yasuda 2004; Calem and Rob 1999; Laeven and Levine 2009). In this article, we focus on the interaction between capital regulation and accounting information quality, and we find that more precise information may weaken the disciplinary effect of capital regulation on risk-taking behavior.

There are also several studies on the implications of accounting measurement for risk-taking behavior. For example, Li (2009) analyzes how different accounting regimes affect banks' risk-taking decisions through capital regulation, and finds that a lower-of-cost-or-market regime is more effective in controlling banks' risk-taking than other regimes. Burkhardt and Strausz (2009) contend that lower-of-cost-or-market accounting may aggravate the asset-substitution problem caused by debt financing. Bertomeu and Magee (2011) show that a shift of accounting information quality driven by an economic downturn may result in more bad loans. Bushman and Williams (2011) find evidence supporting the view that accounting discretion over loan-loss provisioning can have either positive or negative real consequences in disciplining of banks' risk-taking. In contrast to these papers, our model illustrates how accounting information, capital regulation, and market competition interact in their effects on banks' risk-taking behavior.

There are numerous previous studies on the effects of accounting information quality on firms' internal decisions. Some studies show that more detailed information may not be efficient (Arya and Mittendorf 2011; Arya, Glover, and Liang 2004). Similarly, our article shows that the improvement of accounting information quality may not be beneficial. In a capital market setting, Dye and Sridhar (2007) examine the interaction between the choice of accounting information precision and investment decisions under different observability assumptions. In contrast, we examine the role of accounting information quality in a product market setting and we focus on accounting information's effect on banks' risk-taking.

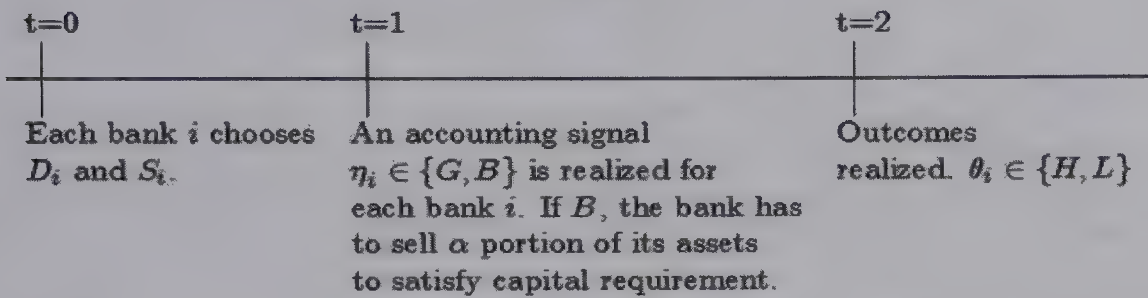
Fewer studies have examined the interactions between capital standards, risk-taking, and accounting rules. Among the few, Besanko and Kanatas (1996) show that when banks resort to issuing new equity in order to satisfy the capital requirement, a more stringent capital requirement may actually lead to more bank failures. In contrast to our article, the key factor driving their results comes from a dilution effect, in which increasing capital standards dilute insiders' ownership, which, in turn, reduces their incentive to exert effort in improving loan quality.

III. MODEL

Setup

We examine a three-date setting in which $N \geq 2$ identical risk-neutral banks compete in a market for deposits. At date 0, each bank i decides on how much deposit funds D_i to obtain and

FIGURE 1
Timeline



chooses the risk level S_i at which it invests these funds in loans. The outcome of all loans of bank i is described by a binary state, $\theta_i \in \{H, L\}$, where H stands for high and L stands for low, and this state is realized at date 2. At date 1, an imperfect accounting signal, η_i, which is informative about the future outcome of the loans, is generated for each bank i and observed publicly. The accounting signal is also binary, $\eta_i \in \{G, B\}$, where G stands for good and B stands for bad.[1] In the case of a bad signal, the bank must sell some of its assets (i.e., loans) to fulfill a capital requirement. Finally, at date 2, the outcome is realized. The timeline of the model is shown in Figure 1.

At date 0, all banks make two decisions simultaneously: the total amount of deposit funds, D_i, and the risk level at which they invest those funds, $S_i \in [0, 1]$. We assume that the banks' choices of S_i and D_i are not perfectly observed by outsiders. This assumption reflects circumstances in practice and makes the model tractable without driving our results. In the robustness checks in Section VI, we illustrate that the model with observable decisions provides qualitatively similar results. The deposit market is represented by an upward-sloping inverse supply curve that yields the equilibrium gross deposit rate, $r_D(D_A)$, as a function of the aggregate bank deposit amount, $D_A = \sum_{j=1}^{N} D_j$. For simplicity, we assume that r_D has the linear functional form $r_D(D_A) = bD_A + \varepsilon$, where $b > 0$, and ε is an unobservable random shock reflecting other factors that influence the deposit rate. We assume $E[\varepsilon] = 0$, and that ε has a support with a positive measure, but sufficiently small. This expression implicitly assumes that deposit amounts are perfect substitutes and increase the gross deposit rate. Because all deposits are fully insured by the Federal Deposit Insurance Corporation (FDIC), the competitive gross deposit rate, r_D, is independent of the individual and aggregate risk of all banks.[2]

We assume that each bank i invests all funds obtained from deposits in bank loans that, in aggregate, have an uncertain outcome, X_i. The outcome of these loans is characterized by the state $\theta_i \in \{H, L\}$, such that the loans are either in a high state (H), in which they yield a high outcome, or in a low state (L), in which they yield a low outcome that we normalize to zero. Neither the bank nor outsiders observe the realized state and outcome until date 2. The risk level of the loans, S_i, affects the expected return of the loans in two ways. First, given the loan amount, D_i, a higher loan risk yields a higher return in the H state. In particular, in the H state, the loans yield a cash flow of $(1 + S_i)D_i$, while the loans yield a zero cash flow in the L state. Second, we follow Boyd and Nicolò

[1] This binary assumption simplifies our analysis without much loss of generality; to verify this, we examined a setting with a continuum of states and accounting signals and found that the main results still hold qualitatively. Detailed analysis of this continuous-state setting is available upon request.

[2] The FDIC insurance assumption, without driving our main results, simplifies our analysis. Even if we assume that deposits are not insured and that r_D depends on the market's conjecture of total risk, banks' returns are only reduced in the H state, and our results remain valid.

(2005) in assuming that the probability that the loans end up in the H state, $P(S_i)$, decreases with their risk. In particular, we assume that $P(S_i)$ follows a linear function, $P(S_i) = 1 - S_i$, where S_i lies in the unit interval.[3] The outcome from the loan investment can be characterized as follows:

$$X_i = \begin{cases} (1+S_i)D_i & \text{if } \theta_i = H, \\ 0 & \text{if } \theta_i = L. \end{cases}$$

At date 2, bank i pays $r_D D_i = (bD_A + \varepsilon)D_i$ to depositors only in the H state. In the L state, the bank obtains a zero cash flow from the loan investment and does not pay depositors because banks have limited liability. Absent any capital requirement examination, bank i would expect a net cash flow of:

$$P(S_i)(1 + S_i - E[r_D])D_i. \tag{1}$$

This expression reflects the basic risk-return trade-off for the bank: a higher level of risk decreases the probability of the H state, but increases the net loan cash flow if the H state is realized. This trade-off makes the expected net cash flow strictly concave in S_i, and ensures an interior maximum at $\frac{E[r_D]}{2}$. Also, notice that if banks were forced to bear the burden of covering defaults (i.e., pay depositors in the L state), then they would expect a net cash flow of $(P(S_i)(1+S_i) - E[r_D])D_i = ((1 - S_i)(1+S_i) - E[r_D])D_i$ and, hence, would optimally choose a risk level of zero. In our model, as a result of limited liability, banks deviate from this "first-best" risk choice and take risk excessively.[4]

At date 1, an imperfect accounting signal, η_i, on the loan performance is generated and observed publicly. The quality of this accounting information is represented by an exogenous parameter, ϕ, which is the probability that the signal generated is correct. That is, $Pr(\eta_i = G|\theta_i = \mathrm{H}) = Pr(\eta_i = B|\theta_i = L) = \phi$. We assume that the accounting signal is imperfectly informative: $\frac{1}{2} < \phi < 1$.

In reality, banks face a capital requirement that is based on accounting measures. This capital requirement requires a bank to maintain a minimum capital ratio, which is calculated as the bank's equity over its risk-weighted assets. In economic downturns, the bank's assets are often impaired while the associated impairment losses reduce the bank's equity value. These two effects jointly result in a lower capital ratio. To fulfill the capital requirement, banks frequently sell risky assets to boost their capital ratio.[5] For instance, First Financial Network planned to sell \$150 million in loan participation from four failed banks in October 2009 (*Business Wire* 2009) and BNP Paribas sold \$96 billion of assets to shore up capital and cut funding needs (Reuters 2011). Consistent with these observations, we assume in our model that if the accounting signal realization is B, then the bank violates the capital requirement and must sell a portion of its risky assets for cash. For simplicity, we assume that the proportion of assets that needs to be sold is a constant, $\alpha \in (0, 1)$, which

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[3] We examined a more general functional form for the probability distribution in a continuous-state setting, and we find that the main results qualitatively remain.

[4] We thank an anonymous referee for bringing up this point. When there is no limited liability, the optimal choice of zero risk is, in fact, a normalization. By adjusting parameters in the model, we could potentially normalize the optimal risk choice to any arbitrary value.

[5] For example, suppose a bank's risky assets are worth \$2 million, its equity is recognized to be \$1 million, and the weight for risky assets in the calculation of the risk-weighted assets is 100 percent. The capital ratio is then 0.5. Assume that, upon a bad accounting signal, the market value of the risky assets declines to \$1.5 million. Then, the assets' value is marked to market and the impairment loss reduces the equity book value to \$0.5 million. The capital ratio after the accounting signal, therefore, declines to 0.33. If the bank then fails the capital examination, then it must take steps to satisfy the regulatory capital requirement. In particular, the bank can sell a part of its risky assets for cash. Suppose the bank sells \$0.5 million of its risky assets for cash. Because cash has zero weight in the calculation of risk-weighted assets, the new risk-weighted assets amount to \$1 million and, as a result, the capital requirement ratio is boosted back to 0.5.

henceforth we refer to as the "assets sales portion." The market price of the bank's assets, $Asset_i^B$, equals the market's conditional expectation of the future value of the loans:

$$Asset_i^B = E[X_i(S_i^c, D_i^c, D_{-i}^c)|B] = \left[\frac{(1-\phi)(1-S_i^c)}{(1-\phi)(1-S_i^c)+\phi S_i^c}\right](1+S_i^c)D_i^c.$$

In this expression, the term in square brackets is the conditional probability of the H state given a B signal, and the rest is the loan outcome in the H state. Note that the assets price, $Asset_i^B$, is only a function of the investors' risk conjectures and, therefore, it is *ex ante* independent of the bank's actual choices. To avoid trivial cases, we assume that once a capital-deficient bank ends up in the L state, the cash proceeds from the assets sale are not sufficient to repay depositors. However, by the virtue of limited liability, the bank is not liable for the outstanding balance. It can be shown that this assumption is satisfied if $\alpha < \frac{1}{2}$ and, thus, we henceforth assume $\alpha \in (0, \frac{1}{2})$. In addition, for expositional purposes, we disregard the bank's option to sell assets after a good signal realization. This is without loss of generality because, as we will show, the bank incurs an endogenous cost when selling the assets and therefore it is not willing to sell unless it is forced.

We can now specify the bank's objective function. At date 0, each bank i chooses the deposit quantity D_i and the loan risk S_i to maximize its expected net cash flow:

$$\max_{S_i, D_i} P_G E[\pi_i|G] + (1-P_G)E[\pi_i|B], \tag{2}$$

where P_η denotes the probability of signal η, $\eta \in \{G, B\}$, and π_i denotes the bank's cash flow net of payments to depositors. Conditional on a G signal, π_i has an expected value of $E[\pi_i|G] = P_{H|G} max\{(1+S_i - E[r_D])D_i, 0\}$, where $P_{H|G}$ denotes the conditional probability of the H state given a G signal. The expressions for all conditional probabilities can be found in Appendix A. In this expression, the maximum operator reflects the fact that the bank has limited liability and, therefore, cannot have a negative terminal value. If the realization of the net cash flow in the H state is positive, then it reflects the loan outcome, $(1 + S_i)D_i$, net of expected payments to depositors, $E[r_D]D_i$. Upon a B signal, π_i has an expected value of:

$$E[\pi_i|B] = P_{H|B} max\left\{(1-\alpha)(1+S_i)D_i + \alpha\ Asset_i^B - E[r_D]D_i, 0\right\}.$$

In this expression, the maximum operator reflects the fact that the bank has limited liability. The bank obtains a positive value only when the H state is realized and the net cash flow is positive. In this expression, the term $(1-\alpha)(1+S_i)D_i$ is obtained from the unsold portion of the loans, the term $\alpha Asset_i^B$ is obtained from the sold portion of the loans, and the term $E[r_D]D_i$ is the payment to depositors.

IV. EQUILIBRIUM

We define the equilibrium in our model as follows:

Definition 1: Equilibrium: a Perfect Bayesian Equilibrium in this game is a triple $\left\{S_i^*, D_i^*, Asset_i^\eta\right\}$ for each bank $i \in \{1, \ldots, N\}$ such that:

- At date 0, each bank i chooses the optimal risk and loan amount, $\left\{S_i^*, D_i^*\right\}$, to maximize its expected future cash flow, $E[\pi_i] = P_G E[\pi_i|G] + (1 - P_G)E[\pi_i|B]$.
- The market price of bank i's assets at date 1 contingent upon the accounting signal, $Asset_i^\eta$, is equal to the market's updated expectation of the loans' outcome:

$$Asset_i^\eta = E[X_i(S_i^c, D_i^c, D_{-i}^c)|\eta], \quad \eta \in \{G, B\},$$

where S_i^c, D_i^c, D_{-i}^c represent the market's conjectures of bank i's risk level, bank i's deposit amount, and the total deposit amount of all other banks, respectively.

- In equilibrium, the market's conjectures of each bank i's risk-taking and investing decisions equal the bank's actual decisions; i.e., $(S_i^c, D_i^c) = (S_i^*, D_i^*)$ for all $i \in \{1,\ldots,N\}$.

We derive the equilibrium as follows. At date 0, each bank i chooses the deposit quantity D_i and the loan risk S_i to solve:

$$\begin{aligned} &\max_{S_i, D_i} P_G P_{H|G} \max\{(1 + S_i - bD_A)D_i, 0\} \\ &\quad + (1 - P_G)P_{H|B} \max\{(1-\alpha)(1+S_i)D_i + \alpha Asset_i^B - bD_A D_i, 0\}. \end{aligned} \tag{3}$$

For expositional purposes, we assume that the realization of the net cash flow in the H state after a G signal is always positive. That is:

$$(1 + S_i - bD_A)D_i > 0. \tag{4}$$

Nevertheless, this condition is always satisfied in the equilibrium (S^*, D^*) characterized in Proposition 1, where $1 + S^* > bND^* = bD_A^*$. We consider two cases. In the first case, the bank obtains a positive net cash flow in state H after a bad accounting signal; in the second case, this net cash flow is negative. In Appendix B, we prove that there exists a threshold $\hat{N}$ such that, if $N < \hat{N}$, then the first case applies, and otherwise, the second case applies.[6] Formally, we must take into consideration the condition:

$$(1-\alpha)(1+S_i)D_i + \alpha Asset_i^B - bD_A D_i > 0, \ \text{for all } N < \hat{N}. \tag{5}$$

We first consider the case in which $N < \hat{N}$. The bank's program can be expressed as:

$$\max_{S_i, D_i} P_G P_{H|G}[(1 + S_i - bD_A)D_i] + (1 - P_G)P_{H|B}[(1-\alpha)(1+S_i)D_i + \alpha Asset_i^B - bD_A D_i]. \tag{6}$$

Taking derivatives with respect to the two choice variables, we obtain two first-order conditions:

$$b(D_{-i} + D_i)D_i - 2[\phi + (1-\phi)(1-\alpha)]S_i D_i + \alpha(\phi - 1)Asset_i^B = 0, \tag{7}$$

$$\left(1 + S_i - \frac{b(D_{-i} + 2D_i)}{1 - \alpha(1-\phi)}\right)(1 - S_i) = 0. \tag{8}$$

In equilibrium, the market's conjectures are true; i.e., $(S_i^c, D_i^c) = (S_i, D_i)$ for all $i \in \{1, 2, \ldots, N\}$. Therefore, the above equations become:

$$\begin{aligned} &D_i\left(b(D_{-i} + D_i) - 2[\phi + (1-\phi)(1-\alpha)]S_i + \alpha(\phi - 1)\left(\frac{(1-\phi)(1-S_i)}{(1-\phi)(1-S_i) + \phi S_i}\right)(1 + S_i)\right) \\ &\quad = 0, \end{aligned} \tag{9}$$

[6] As we discuss later in the article, a bank bears a false-alarm cost when forced to sell assets upon a B signal. As the number of banks increases, the more intense competition erodes the profit margin; when $N \geq \hat{N}$, the profit margin becomes so small that it cannot cover the false-alarm cost, resulting in the bank's insolvency even if the bank ultimately ends up in the H state.

$$\left(1+S_i-\frac{b(D_{-i}+2D_i)}{1-\alpha(1-\phi)}\right)(1-S_i)=0. \tag{10}$$

Solving the system of equations for all banks simultaneously, one can derive the decisions for each bank in equilibrium. Notice that $D_i = 0$ and $S_i = 1$ are obvious solutions to Equations (10) and (9), respectively. However, these solutions do not satisfy the second-order conditions and therefore are discarded. Also, from Equation (10), it is apparent that there is a linear relation between the optimal risk and investment choices. Indeed, solving for S_i, we have $S_i = \frac{b(D_{-i}+2D_i)}{1-\alpha(1-\phi)} - 1$. Therefore, if the size of the loan assets were exogenous, then an increase in the size of a bank's loan assets would imply an increase in their risk. The resulting equilibrium expressions for the assets market price and banks' decisions are stated below in Proposition 1. The equilibrium derivation for the case of $N < \hat{N}$ can be found in Appendix B.

On the other hand, in the second case for $N \geq \hat{N}$, upon a G signal, condition (4) suggests that the bank is solvent. However, upon a B signal, the bank receives a non-positive cash flow even if it ends up in the H state; therefore, a B accounting signal announces the bank's insolvency. The bank's program, thus, reduces to:

$$\max_{S_i,D_i} P_G P_{H|G}(1+S_i-bD_A)D_i. \tag{11}$$

Taking derivatives with respect to the two choice variables, we obtain two first-order conditions:

$$[b(D_{-i}+D_i)-2S_i]\ D_i=0, \tag{12}$$

$$(1+S_i-bD_{-i}-2bD_i)(1-S_i)=0. \tag{13}$$

As in the previous case, solving the system of equations for all banks simultaneously, we obtain the expressions for the equilibrium deposit amounts and loan risks. We can show that the equilibrium is always unique and symmetric. Henceforth, we omit the firm index and denote the equilibrium strategy profile by $\{S^*, D^*, Asset^B\}$. Proposition 1 describes the equilibrium:

Proposition 1: There exists a unique and symmetric equilibrium in which:

- each bank makes the optimal risk-taking decision S^* and loan decision D^* given by:

$$S^* = \begin{cases} \dfrac{-k_2-\sqrt{k_2^2-4k_1k_3}}{2k_1} & \text{if } N<\hat{N} \\ \dfrac{N}{N+2} & \text{if } N \geq \hat{N} \end{cases}, \quad \text{and}$$

$$D^* = \begin{cases} \dfrac{1-\alpha(1-\phi)}{b\ (N+1)}(1+S^*) & \text{if } N<\hat{N} \\ \dfrac{2}{b\ (N+2)} & \text{if } N \geq \hat{N} \end{cases},$$

where the coefficients (k_1, k_2, k_3) are defined as functions of ϕ, α, and N:

$$k_1 = (N+2)(1-2\ \phi) + \alpha(1-\ \phi)[(N+3)\phi - 1],$$

$$k_2 = [1-\alpha(1-\phi)][(3N+2)\phi - 2(N+1)],$$

$$k_3 = (1-\phi)[N-\alpha(1-\phi)(2N+1)],$$

and $\hat{N} > 0$ is a threshold such that at $N = \hat{N}$, we have in equilibrium: $(1-\alpha)(1+S^*)D^* + \alpha\ Asset_i^B - bND^{*2} = 0$;

- upon a bad accounting signal, the bank's assets market price, $Asset^B$, is given by:

$$Asset^B = \frac{(1-\phi)(1-S^*)}{(1-\phi)(1-S^*)+\phi S^*}(1+S^*)D^*.$$

The unique and symmetric equilibrium adopts two different characterizations, depending on whether the number of banks is below or above a threshold, $\hat{N}$. When $N < \hat{N}$, the equilibrium investment and risk decisions are contingent on the accounting information quality, ϕ, and the assets sales portion, α. However, when $N > \hat{N}$, ϕ and α do not affect the equilibrium investment and risk decisions. In the following subsections, we will examine and explain the results in these two cases.

Case of $N < \hat{N}$

When the number of banks is sufficiently small ($N < \hat{N}$), banks' risk-taking decisions are contingent on both the accounting information quality and the capital requirement. By examining the comparative static properties of the equilibrium presented in Proposition 1, we find that a bank's risk-taking incentives are disciplined by a higher asset sale portion, α. This is consistent with the intention of bank regulators in setting a capital requirement to induce less aggressive risk decisions. However, our analysis also demonstrates that an improvement in the quality of accounting information actually heightens a bank's risk-taking incentives. We summarize these results in the following proposition:

Proposition 2: When $N < \hat{N}$, we have $\frac{\partial S^*}{\partial \phi} > 0$, $\frac{\partial S^*}{\partial \alpha} < 0$, and $\frac{\partial^2 S^*}{\partial \phi \partial \alpha} > 0$.

The results in Proposition 2 are driven by the trade-off between two effects: a *false-alarm-cost* effect and a *deposit-market* effect. To understand these effects and the trade-off between them, recall the bank's objective function in Equation (3):

$$P_G P_{H|G} max\{(1+S_i-bD_A)D_i, 0\} \\ + (1-P_G)P_{H|B} max\{(1-\alpha)(1+S_i)D_i + \alpha\ Asset_i^B - bD_A D_i, 0\}.$$

According to Equation (4), the realization of the net cash flow in the H state upon a G signal is always positive. In addition, when $N < \hat{N}$, the bank obtains a positive net cash flow in state H upon a B signal. Therefore, the bank's objective function becomes:

$$P_G P_{H|G}(1+S_i-bD_A)D_i + (1-P_G)P_{H|B}\{(1-\alpha)(1+S_i)D_i + \alpha\ Asset_i^B - bD_A D_i\}$$

$$= P_G P_{H|G}(X_i^H - E[r_D]D_i) + (1-P_G)P_{H|B}\{(1-\alpha)X_i^H + \alpha\ Asset_i^B - E[r_D]D_i\},$$

where for convenience we denote the outcome of the loans in the H state, $(1+S_i)D_i$, as X_i^H, and the expected gross deposit rate, bD_A, as $E[r_D]$. The above last expression, then, can be rewritten as:

$$\underbrace{P_H(X_i^H - E[r_D]D_i)}_{\text{Bank's expected cash flow with no assets sales}} \quad -P_B P_{H|B} \quad \underbrace{\alpha(X_i^H - Asset_i^B)}_{\text{False-alarm cost of assets sales}} \quad .$$

The first component in the bank's objective function coincides with Expression (1), the expected net cash flow the bank would obtain if there were no forced asset sales. This term shows that the bank expects to repay $E[r_D]D_i$ to depositors only if the H state is realized. As a consequence, the optimal level of risk implied by this first component is affected by an asset-substitution problem between the bank and the depositors. A larger expected payment $E[r_D]D_i$ reduces the marginal benefit of increasing the probability of the H state, P_H. As a result, banks turn to riskier investments to achieve a higher loan margin (i.e., a larger $X_i^H - E[r_D]D_i$).

The second component in the bank's objective function represents an endogenous cost stemming from the sale of assets. The magnitude of the cost corresponds to the difference between the proceeds from assets sales, $\alpha Asset_i^B$, and the cash flows from the loan assets obtained in the H state, αX_i^H. This cost is not due to the illiquidity in the assets market as the assets in our model are sold at their fair price. It is borne only by a bank that receives a pessimistic accounting signal, but ultimately stays solvent. It arises because when the bank sells its assets, neither the bank nor the market knows the future outcome. Therefore, the fair price that the market offers reflects the expected cash flows considering the possibility of both good and bad outcomes. If a bad outcome is realized, then the bank becomes insolvent, all the sale proceeds are paid to the depositors and, therefore, the net value of the assets sale proceeds for the bank is zero. However, if a good outcome is realized, then the cash flow generated by the investment, αX_i^H, is actually larger than the proceeds obtained from selling the assets, $\alpha Asset_i^B$. Therefore, the "early" assets sale results in an endogenous cost. This cost is incurred only if both a bad signal and the H state are realized (i.e., when the accounting system generates a false alarm); from the perspective of the banks, it reflects an economic inefficiency arising from the imperfect accounting information. We call this cost the *false-alarm cost* and denote it by c_S, where $c_S = \alpha(X_i^H - Asset_i^B)$.

To illustrate how the false-alarm cost affects the optimal risk choice, we rewrite the objective function in the following way:

$$P_H(X_i^H - E[r_D]\ D_i) - P_H(1-\phi)c_S. \tag{14}$$

Like the expected deposit payment $E[r_D]D_i$, the false-alarm cost is only incurred in the H state. Therefore, it plays an analogous role to the one played by $E[r_D]D_i$ in influencing the bank's risk decision. That is, an increase of c_S lowers the bank's net cash flow in the H state, which encourages the bank to pursue risky projects more aggressively.

Improving the quality of accounting information affects a bank's risk-taking decision in two ways. On one hand, improving the information quality reduces the size of the expected false-alarm cost, $P_H(1 - \phi)c_S$, which is a convergence of two opposing forces: an increase in information quality increases the size of the false-alarm cost, but reduces the probability that a false alarm actually incurs. Indeed, as accounting information quality improves, the chance that a bank ends up in the H state after obtaining a bad signal decreases, which makes the false-alarm cost less likely to be incurred. However, the lower H-state chance also reduces the assets sales price, and that, in turn, yields a higher false-alarm cost. On balance, the decrease in probability dominates, resulting in a lower expected false-alarm cost. This lower false-alarm cost mitigates the asset-substitution problem between the bank and the depositors and, as a result, induces the bank to take less risk. That is, keeping the bank investment decision fixed, increasing ϕ directly restrains the bank's risk-taking behavior. We call this disciplinary role of accounting information the *false-alarm cost effect*. On the other hand, increasing the quality of accounting information also affects the bank's investment decisions. The decrease of the expected false-alarm cost

induced by an increase in information quality increases the bank's marginal investment return. As a result, each bank responds by increasing its investment amount, D_i. The resulting increase in aggregate investment leads to a higher deposit rate. The higher deposit rate, in turn, exacerbates the asset-substitution problem between the bank and the depositors, and motivates the bank to be more aggressive in risk-taking. We call this risk-inducing role of accounting information the *deposit-market effect*.

In the trade-off between the two opposing effects of heightened information quality, the risk-inducing deposit-market effect more than offsets the disciplinary false-alarm cost effect. As a result, increasing accounting information quality motivates banks to take more risk. Indeed, this result illustrates that when examining the relation between information quality and banks' risk-taking decisions, it is important to consider the endogeneity of investment decisions. Improving the quality of accounting information improves the discriminating efficiency of the capital requirement policy, thereby reducing the chances of forcing solvent banks to liquidate assets. This restrains a bank's risk-taking incentive because the associated efficiency improvement raises the charter values for banks, and higher charter values motivate banks to make more prudent decisions. However, the influence of accounting information quality is reversed once we consider the endogeneity of investment decisions. This is because banks respond to the improvement in discriminating efficiency by expanding investments, which raises the deposit rate. A higher deposit rate, in return, lowers the charter value and results in excessive risk-taking.

The disciplinary effect of an increase in the assets sales has a similar interpretation. If bank regulators raise the capital requirement ratio, resulting in a higher α, then this affects a bank's risk-taking decision in two ways. First, it forces a bank to sell more assets upon a bad signal to satisfy the capital requirement, which, in turn, leads to a higher expected false-alarm cost. Taking investment decisions as exogenous, a higher expected false-alarm cost strengthens the bank's asset-substitution incentive and encourages the bank to take risk more aggressively. However, the larger expected false-alarm cost leads to a lower marginal investment return. As a result, if we consider investment decisions to be endogenous, then banks tend to invest less and compete less aggressively in the deposit market. Therefore, a higher α softens the competition in the deposit market, thereby reducing the deposit rate. A lower deposit rate mitigates the asset-substitution problem between the bank and the depositors, and induces the bank to take less risk. Overall, this latter disciplinary effect dominates the former risk-inducing effect and, as a result, a higher α restrains banks from aggressive risk-taking. That is, requiring banks to hold more capital not only builds an extra layer of protection for depositors, but also discourages banks from taking excessive risk.

Proposition 2 also shows that $\frac{\partial^2 S^*}{\partial \phi \partial \alpha} > 0$. That is, forcing capital-deficient banks to sell more assets can reinforce the risk-inducing effect of accounting information. The key driving force is that when banks are forced to sell a larger fraction of their assets to satisfy the regulatory capital requirement, the false-alarm cost associated with the assets sales is also more substantial. Therefore, an improvement in the accounting information quality leads to a greater reduction in the false-alarm cost and further intensifies the competition in the deposit market, which causes banks to take risk more aggressively.

Case of $N \geq \hat{N}$

When the number of banks is larger than $\hat{N}$, banks' investment and risk decisions are no longer affected by information quality or the assets sales portion. We state this result formally in the following proposition:

Proposition 3: When $N \geq \hat{N}$, a bank's investment and risk-taking decisions are independent of ϕ and α.

From the expressions for the equilibrium banks' decisions stated in Proposition 3, one can see that the risk taken by each bank is increasing in the number of banks in the market, N, and tends asymptotically to 1, the maximum level of risk, as $N \to \infty$. However, the investment of each bank decreases with the number of banks, as they split the deposit market, and tends to 0 as $N \to \infty$, as each bank becomes infinitesimally small. Nevertheless, the aggregate investment increases with the number of banks and tends to a constant $\frac{2}{b}$ as $N \to \infty$. The limit case as the number of banks approaches infinity is, in fact, the case of a perfectly competitive deposit market and deserves a formal statement, which we provide in the following corollary:

Corollary 1: In the case of perfect competition, each bank makes the equilibrium risk-taking and loan decisions (S^*, D^*) that satisfy $S^* = 1$ and $D_A^* = lim_{N\to\infty} ND^* = \frac{2}{b}$.

Our results for the case with a sufficiently large number of banks (i.e., the case of $N \geq \hat{N}$, including the perfect competition case $N \to \infty$) extend the results of Allen and Gale (2000). Allen and Gale (2000) study a similar setting with N banks competing in the same market, but in their model, banks are not subject to a capital requirement examination and accounting information plays no role. As in this study, they also find that banks choose to take more risk as N increases, and that banks choose the maximum level of risk in a perfectly competitive market. Our contribution is to state that harsher competition induces banks to become more aggressive in risk-taking even in the presence of a capital requirement examination. Moreover, we show that neither capital requirement nor information quality has any effect on the banks' decisions beyond a certain level of competitiveness.

To understand the intuition underlying this result, notice that the payoff for a bank that receives a bad signal and ends up in the H state is $(X_i^H - r_D D_i) - c_S$. As the number of banks increases, the increasing competition erodes the profit margin $X_H - r_D D_i$, and in the case of perfect competition, the profit margin is reduced to zero. However, the false-alarm cost c_S remains positive as N approaches infinity. This is because the false-alarm cost depends on the difference between the proceeds from assets sales and the cash flows from the loan assets obtained in the H state, neither of which is net of the interest payment; therefore, it is not affected by the profit margin and remains positive. When $N \geq \hat{N}$, upon a bad signal, the profit margin $X_H - r_D D_i$ cannot cover the false-alarm cost, resulting in the bank's insolvency even if the bank eventually ends up in the H state. As a result, the bank only cares about its payoff in the case of receiving a good signal. The net cash flow obtained in the H state decreases with N as the loan profit margin decreases and, therefore, the bank becomes more aggressive in risk-taking in trying to regain some of that margin.

Our analysis for both cases, $N < \hat{N}$ and $N \geq \hat{N}$, illustrates the interaction between interbank competition and accounting information quality and their effects on banks' risk-taking behavior. Bank regulators may believe that competition should be restricted by regulation to enhance bank stability. Separately, better accounting disclosure is often posited as an important market disciplining device for banks. However, our article shows that there is an interaction between the two mechanisms: improving information quality may actually increase risk-taking in an environment with mild competition, while it may have no effect on risk decisions in an environment with fierce competition. Therefore, our results imply that these two mechanisms cannot be evaluated in isolation and that regulators need to consider the interaction between them.

V. EXTENSION: ACCOUNTING CONSERVATISM

In this section, we consider an extension of our main setting that incorporates accounting conservatism. To study how accounting conservatism affects our results, we model conservatism by assuming that the conditional probabilities of observing a good or a bad signal for a certain state of the loan are as follows: $\Pr(\eta_i = G|\theta_i = H) = \phi - \lambda$ and $\Pr(\eta_i = B|\theta_i = L) = \phi + \lambda$, where $\phi \in [½, 1]$

and $\lambda \in [0, 1-\phi]$. The parameter ϕ measures the quality of the information as before, and the parameter λ captures the level of conservatism. We follow previous studies, such as Chen, Hemmer, and Zhang (2007), Gigler, Kanodia, Sapra, and Venugopalan (2009), Gao (2013), and Nan and Wen (2014), in modeling conservatism as shifting the conditional distribution of the accounting signal toward the bad signal, making the observation of a bad signal less informative. We present the results of our analysis in Proposition 4:

Proposition 4: There exists an N_c such that:

(i) when $N < N_c$, $\frac{\partial S^*}{\partial \phi} > 0$ and $\frac{\partial S^*}{\partial \lambda} < 0$; and
(ii) when $N \geq N_c$, S^* is independent of ϕ and λ.

We find that the effect of information quality on banks' risk decisions generally does not change qualitatively with the presence of accounting conservatism. In particular, we still find that when the number of banks is small ($N < N_c$), the equilibrium risk chosen by banks strictly increases in the information quality, and that risk decisions are not affected when the number of banks is large ($N \geq N_c$). Moreover, in the latter case, the level of conservatism does not affect risk decisions either. However, the level of conservatism does affect risk decisions when the number of banks is small ($N < N_c$). In particular, a more conservative accounting system (higher λ) decreases the risk taken by banks. That is, conservatism plays a disciplinary role. Given the results in our main setting, the effect of conservatism on the risk decisions is quite intuitive. As mentioned above, a more conservative information system generates a downward bias on the signal, making a bad signal less informative about the underlying state of the loans. Since for the capital requirement examination purpose, the informativeness of the accounting signal is only relevant upon the realization of a bad signal, conservatism effectively reduces the quality of information. Therefore, making the accounting information more conservative in our setting is effectively equivalent to lowering the quality of information and, thus, induces banks to take less risk, as we know from the results in the main setting.

VI. ROBUSTNESS AND CAVEATS

The results illustrated so far in this article are obtained under some simplifying assumptions. Perhaps the two main simplifying assumptions made in the model are: (i) the non-perfect observability of the banks' decisions by the market, and (ii) the exogenous and constant assets sales portion α. To assess the robustness of our results to the relaxation of the former assumption, we examine an alternative specification to the main setup that assumes that the market can perfectly observe both the investment and risk decisions of each bank. This specification of the model leads to a high-degree polynomial in S_i that can only be analyzed numerically. However, we see a similar pattern of increasing equilibrium risk decisions with increasing information quality. This allows us to conclude with some confidence that the unobservability assumptions in isolation do not seem to drive our results.

We also analyze the effect of an endogenous α on our results by assuming that after the accounting signals are released, each bank that obtains a bad signal sells a portion of its assets such that the capital ratio satisfies the capital requirement. This model specification is also difficult to examine analytically. However, numerical simulations facilitate the analysis and show that equilibrium risk decisions are still increasing in information quality for any capital requirement above a certain threshold. Below this threshold, however, risk decisions can decrease with information quality, especially when the number of firms is small.

We also examine the simultaneous relaxation of both simplifying assumptions. That is, we introduce the perfect observability of banks' decisions and the endogeneity of α in the same setting.

Since, in our opinion, arguing that risk decisions are perfectly observable is quite unrealistic, we first examine a setting with an endogenous α, a perfectly observable D_i, and a non-perfectly observable S_i. Numerical simulations show that the qualitative nature of the results is similar to those obtained with an endogenous α and unobservable decisions. That is, we still find a positive relation between banks' risk decisions and information quality for a capital requirement larger than a threshold and the opposite result below this threshold. For completeness, we also examine the most extreme case: an endogenous α and perfectly observable investment and risk decisions. In this case, one can still obtain a positive relation between risk and information quality for capital requirements. Overall, the numerical analyses lead us to believe that the positive relation between risk and information quality described in Proposition 1 is quite robust.

There are aspects of the real world that we do not reflect in our model, but that might potentially affect the results if considered. One such factor is that banks have the possibility of satisfying the capital requirement by raising capital through the issuance of new equity. Another such factor, examined by Boyd and Nicolò (2005), is allowing the risk level to be privately selected by borrowers, but indirectly induced by banks through the offer of menus of contracts. We analyzed both factors separately and were able to show that the revised models yield qualitatively similar results to the ones in Proposition 1.

Finally, although distressed banks sell risky assets with the purpose of increasing their capital ratio, ironically, it is often observed that after obtaining the cash proceeds, they immediately pay bonuses to top executives and/or dividends to their shareholders. Regulators have taken actions to restrict this kind of "cash out," and investors can potentially claw back part of these executive bonuses through litigation. Nevertheless, it may be instructive to study the case in which at least a portion of the sales proceeds is appropriated by the decision makers in the bank. We analyzed such a setting and found that our results in the main setting still remain when the information quality ϕ is low and/or the proportion of appropriated cash proceeds is small. However, when ϕ is sufficiently high and the proportion of appropriated cash proceeds is high, the introduction of this "cash out" makes the bank's risk-taking level decrease in the information quality. Intuitively, when the information quality is high, the assets price upon a bad signal is lower than when accounting information is noisy, which, in turn, diminishes the size of the benefit from "cash out." This, in turn, motivates the bank to take less risk. As the cash-out benefit joins the false-alarm cost effect in disciplining banks' risk-taking, the trade-off with the deposit-market effect becomes more balanced, and that yields a non-monotonic relation between the information quality and the risk taken by the bank.[7]

VII. CONCLUSIONS

We study the interaction between interbank competition and accounting information quality and their effects on banks' risk-taking behavior. We identify a false-alarm cost of assets sales for banks. This cost, together with the imperfect competition among the banks, plays an important role in the relation between accounting information quality and the banks' risk-taking. We find that an improvement in the quality of accounting information may induce banks to take more risk when the competition is less intense. Bank regulators may believe that competition should be restricted by regulation to enhance bank stability. Separately, better accounting disclosure is often posited as an important market disciplining device for banks. Our article shows that there is an interaction between the two mechanisms, where improving information quality actually increases risk-taking with mild competition, while it has no effect under fierce competition. The results imply that these mechanisms cannot be evaluated in isolation.

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[7] Detailed analyses of our robustness checks are available upon request.

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APPENDIX A
Derivations of Bayesian Probabilities

The conditional probability of the H state given a G signal is $P_{H|G} = \frac{\phi(1-S_i)}{\phi(1-S_i)+(1-\phi)S_i}$.
The conditional probability of the H state given a B signal is $P_{H|B} = \frac{(1-\phi)(1-S_i)}{(1-\phi)(1-S_i)+\phi S_i}$.
The probability of a G signal is $P_G = \phi(1 - S_i) + (1 - \phi)\, S_i$.
The probability of a B signal is $P_B = 1 - P_G$.

APPENDIX B
Proofs

Proof of Proposition 1

Proof: Let us assume first that the bank is solvent when it receives a B signal and ends up in the H state. That is, there exists a threshold $\hat{N}$ such that for $N < \hat{N}$, we must have in equilibrium:

$$(1-\alpha)(1+S_i)D_i + \alpha Asset_i^B - D_i bD_A > 0. \tag{15}$$

In this case, from Equation (9) in the main text, we can state:

$$bD_A - 2[\phi + (1-\phi)(1-\alpha)]S_i + \alpha[\phi(\delta+1) - 1]\left(\frac{(1-\phi)(1-S_i)}{(1-\phi)(1-S_i)+\phi S_i}\right)(1+S_i) = 0.$$

Solving for S_i in the above equation, we obtain two solutions. One of them is obviously not a feasible solution because it is either negative or larger than 1 and, therefore, is discarded. For brevity, we call the feasible solution $S_i(D_A)$. Notice that $S_i(D_A)$ is only contingent on the basic parameters and the aggregate investment D_A. This implies that for a given aggregate investment, there is a unique interior risk decision. From Equation (10) in the main text, we can state $(1 + S_i(D_A))(1 - \alpha\,(1 - \phi)) - b(D_A + D_i) = 0$.

From the previous derivation, we know that S_i is only contingent on D_A; thus, we can aggregate this equation across all banks and obtain $N(1+S_i)(1-\alpha\,(1-\phi)) - b(N+1)D_A = 0$. Solving for D_A, we obtain $D_A = \frac{N(1+S_i)(1-\alpha(1-\phi))}{b(N+1)}$. Substituting this expression into the expression for $S_i(D_A)$ gives us a quadratic equation, which has a unique solution between 0 and 1, and is given by:

$$S^* = \frac{-k_2 - \sqrt{k_2^2 - 4k_1k_3}}{2k_1}. \tag{16}$$

Therefore, given our assumptions on (α,ϕ,N), there exists a unique interior equilibrium (S^*, D^*). All we need to show is that the second-order conditions are also satisfied and that the corner solutions for S_i are never optimal. To show this, we start with showing that the second-order conditions are satisfied locally at the interior optimal choices:

$$\frac{\partial^2 E[\pi_i]}{\partial S_i^2} = -[1-\alpha(1-\phi)]D_i < 0,$$

$$\frac{\partial^2 E[\pi_i]}{\partial D_i^2}\frac{\partial^2 E[\pi_i]}{\partial S_i^2} - \frac{\partial^2 E[\pi_i]}{\partial D_i \partial S_i}\frac{\partial^2 E[\pi_i]}{\partial S_i \partial D_i} = -4b[1-\alpha(1-\phi)]D_i(1-S_i)$$

$$-\{b(D_A + D_i) - [1-\alpha(1-\phi)]S_i\}^2 > 0.$$

The first equation above is obviously satisfied. The second equation, using Equation (10), can be reduced to the condition $S^* > \frac{N-3}{N+5}$. This condition is always satisfied for all $N \geq 2$, $0 \leq \alpha \leq ½$, and $½ \leq \phi \leq 1$. Therefore, we can state that the interior solution is a local maximum. Since there is only one interior solution, we just need to prove that $S_i = 0$ and $S_i = 1$ are not optimal. For $S_i = 1$, we have that $E[\pi_i] = 0$ and, therefore, it cannot be optimal. For $S_i = 0$, it can be proven that $\frac{\partial E[\pi_i]}{\partial S_i}|_{S_i=0} > 0$. Thus, $S_i = 0$ cannot be the optimal solution either. Therefore, we have proven that the interior solution for S_i given (16) is the absolute maximum for $N < \hat{N}$.

Substituting (S^*, D^*) into Constraint (15), it can be reduced to two conditions: $S^* < \frac{(1-\phi)^2}{1-2\phi(1-\phi)}$ or $N - \frac{1}{\alpha}\frac{1-\phi-[1-(2-\alpha)\phi]S^*}{[1-2(1-\phi)\phi]S^*-(1-\phi)^2} < 0$. The first condition can be further reduced to $N < \hat{N}_1$, where $S^*(\hat{N}_1) = \frac{(1-\phi)^2}{1-2\phi(1-\phi)}$, since S^* is strictly increasing in N. In addition, it can be verified that when $N > \hat{N}_1$, the *LHS* of the second condition is strictly increasing in S^* and, hence, increasing in N. Therefore, there exists another threshold $\hat{N}_2$, such that $\hat{N}_2 - \frac{1}{\alpha}\frac{1-\phi-[1-(2-\alpha)\phi]S^*(\hat{N}_2)}{[1-2(1-\phi)\phi]S^*(\hat{N}_2)-(1-\phi)^2} = 0$. Moreover, when $N < \hat{N}_2, N - \frac{1}{\alpha}\frac{1-\phi-[1-(2-\alpha)\phi]S^*}{[1-2(1-\phi)\phi]S^*-(1-\phi)^2} < 0$. Define $\hat{N} = min(\hat{N}_1, \hat{N}_2)$. Combining these analyses, we have when $N < \hat{N}$, Constraint (15) is satisfied. However, when $N \geq \hat{N}$, Constraint (15) is violated, which makes the bank insolvent when the bank receives a bad signal, but ends up in the H state. Solving the first-order conditions in this case gives $S^* = \frac{N}{N+2}$ and $D^* = \frac{2}{b\ (N+2)}$.

We can prove that the second-order conditions for the case of $N \geq \hat{N}$ are also satisfied in an analogous way. ∎

Proof of Proposition 2

Proof: When $N < \hat{N}$, the bank is solvent when the bank receives a B signal, but ends up in the H state. In that case, $\frac{\partial S^*}{\partial \phi}$ can be derived as follows:

$$\frac{\partial S^*}{\partial \phi} = -\frac{\frac{\partial k_1}{\partial \phi}\ S^{*2} + \frac{\partial k_2}{\partial \phi}\ S^* + \frac{\partial k_3}{\partial \phi}}{2\ k_1\ S^* + k_2}.$$

First, the denominator $2k_1S^* + k_2$ can be simplified as $\sqrt{k_2^2 - 4k_1k_3}$, which is positive given that the equilibrium exists. Hence, the sign of $\frac{\partial S^*}{\partial \phi}$ is solely determined by the numerator. With a few algebra steps, we can verify $\frac{\partial k_1}{\partial \phi}S^{*2} + \frac{\partial k_2}{\partial \phi}S^* + \frac{\partial k_3}{\partial \phi} < 0$ and, as a result, $\frac{\partial S^*}{\partial \phi} > 0$.

Similarly, $\frac{\partial S^*}{\partial \alpha}$ can be derived as follows:

$$\frac{\partial S^*}{\partial \alpha} = -\frac{\frac{\partial k_1}{\partial \alpha}S^{*2} + \frac{\partial k_2}{\partial \alpha}S^* + \frac{\partial k_3}{\partial \alpha}}{2k_1S^* + k_2}.$$

As shown before, the numerator is positive given that the equilibrium exists. Hence, the sign of $\frac{\partial S^*}{\partial \alpha}$ is solely determined by the numerator. With more algebra steps, we can verify $\frac{\partial k_1}{\partial \alpha} S^{*2} + \frac{\partial k_2}{\partial \alpha} S^* + \frac{\partial k_3}{\partial \alpha} > 0$ and, as a result, $\frac{\partial S^*}{\partial \alpha} < 0$.

$\frac{\partial^2 S^*}{\partial \phi \partial \alpha}$ can be derived as:

$$\frac{\partial^2 S^*}{\partial \phi \partial \alpha} = \frac{m_1 S^{*3} + m_2 S^{*2} + m_3 S^* + m_4}{(2k_1 S^* + k_2)^2},$$

where:

$$m_1 = 2\left(\frac{\partial k_1}{\partial \alpha}\frac{\partial k_1}{\partial \phi} - k_1 \frac{\partial^2 k_1}{\partial \alpha \partial \phi}\right), m_2 = 2\left[\frac{\partial k_2}{\partial \alpha}\frac{\partial k_1}{\partial \phi} - k_1\left(\frac{\partial^2 k_2}{\partial \alpha \partial \phi} + \frac{\partial k_1}{\partial \alpha}\frac{\partial S^*}{\partial \phi}\right)\right] + \frac{\partial k_1}{\partial \alpha}\frac{\partial k_2}{\partial \phi} - k_2 \frac{\partial^2 k_1}{\partial \alpha \partial \phi},$$

$$m_3 = 2\left[\frac{\partial k_3}{\partial \alpha}\frac{\partial k_1}{\partial \phi} - \left(k_1 \frac{\partial^2 k_3}{\partial \alpha \partial \phi} + k_2 \frac{\partial k_1}{\partial \alpha}\frac{\partial S^*}{\partial \phi}\right)\right] + \frac{\partial k_2}{\partial \alpha}\frac{\partial k_2}{\partial \phi} - k_2 \frac{\partial^2 k_2}{\partial \alpha \partial \phi}, \text{ and}$$

$$m_4 = \frac{\partial k_3}{\partial \alpha}\left(\frac{\partial k_2}{\partial \phi} + 2k_1 \frac{\partial S^*}{\partial \phi}\right) - k_2\left(\frac{\partial^2 k_3}{\partial \alpha \partial \phi} + \frac{\partial k_2}{\partial \alpha}\frac{\partial S^*}{\partial \phi}\right).$$

With more algebra steps, we can verify $m_1 S^{*3} + m_2 S^{*2} + m_3 S^* + m_4 > 0$ and, as a result, $\frac{\partial^2 S^*}{\partial \phi \partial \alpha} > 0$. ■

Proof of Proposition 3

Proof: The proof is directly derived from Proposition 1 for the case $N \geq \hat{N}$. ■

Proof of Corollary 1

Proof: Perfect competition is a special case for $N \geq \hat{N}$. When N goes to infinity, $S^* = 1$ and $D^* = 0$. The total deposit amount becomes $D^A = \frac{2}{b}$. Hence, S^* is independent of either α or ϕ. In this case, the second-order conditions become:

$$\frac{\partial^2 E[\pi_i]}{\partial S_i^2} = \lim_{N \to \infty} - \frac{4\phi}{b(N+2)} = 0,$$

$$\frac{\partial^2 E[\pi_i]}{\partial D_i^2}\frac{\partial^2 E[\pi_i]}{\partial S_i^2} - \frac{\partial^2 E[\pi_i]}{\partial D_i \partial S_i}\frac{\partial^2 E[\pi_i]}{\partial S_i \partial D_i} = \lim_{N \to \infty} \frac{12\phi^2}{(N+2)^2} = 0.$$

Hence, the second-order conditions are satisfied for any N arbitrarily large and, by continuity, they are also satisfied when N goes to infinity in the case of perfect competition. ■

Proof of Proposition 4

Proof: Following similar steps as in Proposition 1, we can show that there exists a threshold N_c, such that when $N < N_c$, the bank is solvent when the bank receives a bad signal, but ends up in the H state. When $N \geq N_c$, the bank is always insolvent upon a bad signal. In this case, solving the first-order conditions gives $S^* = \frac{N}{N+2}$ and $D^* = \frac{2}{b(N+2)}$, which is independent of accounting information quality ϕ and conservatism λ. When $N < N_c$, solving first-order conditions gives $S^* = \frac{-l_2 - \sqrt{l_2^2 - 4l_1 l_3}}{2l_1}$, where l_1, l_2, l_3 are functions of $(\lambda, \phi, N, \alpha)$. By computing the derivatives, it is straightforward to verify that $\frac{\partial S^*}{\partial \phi} > 0$ and $\frac{\partial S^*}{\partial \lambda} < 0$. ■

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Macroeconomic Consequences of Accounting: The Effect of Accounting Conservatism on Macroeconomic Indicators and the Money Supply

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ABSTRACT: This study investigates the macroeconomic consequences of firm-level accounting conservatism. Consistent with conditional conservatism extending to the aggregate level, I demonstrate that annual estimates of aggregate corporate profits and gross domestic product compiled by the U.S. Bureau of Economic Analysis are more sensitive to negative aggregate news than to positive aggregate news. Next, I estimate the dollar value impact of conservatism on measurements of macroeconomic fundamentals. Finally, I show that incorporating the dollar value impact of conservatism increases the explanatory power of a monetary policy reaction function that describes U.S. Federal Reserve interest rate decision behavior. These results suggest that accounting can impact social welfare by altering the measurement attributes of key macroeconomic indicators and by shaping monetary policy decisions that regulate the money supply.

Keywords: *accounting conservatism; aggregate corporate profits; monetary policy; money supply.*

JEL Classifications: *M41; E43; E52.*

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I. INTRODUCTION

This study investigates the macroeconomic consequences of firm-level accounting conservatism. Accounting conservatism has been defined as the tendency to require a higher degree of verification for recognizing gains as compared to losses (Basu 1997; Watts 2003a). Using firm-level stock returns as a proxy for news, Basu (1997) provides evidence that this asymmetric verification requirement results in conditionally conservative firm-level earnings that are more sensitive to bad news than to good news. A large subsequent literature asserts that firms report conservatively in order to minimize contracting costs, litigation risk, and taxes. Firm-level earnings could also be conservative in response to accounting standards, securities regulation, and pressure from external auditors (Watts 2003a, 2003b).

I examine whether firm-level accounting conservatism aggregates to alter the measurement of macroeconomic indicators and influence monetary policy decisions. First, I investigate whether the summation of individual firm earnings results in a conditionally conservative aggregate corporate profits signal. Specifically, I adapt the Basu (1997) asymmetric timeliness framework to examine the time-series behavior of annual estimates of aggregate corporate profits as compiled by the U.S. Bureau of Economic Analysis (BEA), an agency of the U.S. Department of Commerce. Consistent with the existence of conditional conservatism at the aggregate level, the results indicate that aggregate corporate profits from 1929 to 2008 are more sensitive to negative aggregate news than to positive aggregate news.

Identifying conditional conservatism within aggregate corporate profits is important for multiple reasons. First, census data indicate that publicly traded firms constitute only 1 percent of all U.S. firms (Davis, Haltiwanger, Jarmin, and Miranda 2006). Because the aggregate corporate profits measure compiled by the Bureau of Economic Analysis includes the earnings of both public and private firms, my study helps identify how firm-level accounting conservatism influences measurements of the economic performance of the entire U.S. corporate sector.

Second, and perhaps most importantly, aggregate corporate profits are a significant component of U.S. gross domestic product (GDP). In 2008, aggregate corporate profits totaled $1.2 trillion, which amounted to 9 percent of U.S. GDP for the year. GDP is a closely watched macroeconomic indicator that provides a summary measure of national economic conditions (Bureau of Economic Analysis [BEA] 2008). Moreover, GDP measurements influence decisions made by policy makers, firms, investors, and households (BEA 2002). Accordingly, I examine whether the influence of firm-level accounting conservatism extends beyond aggregate corporate profits to alter the measurement attributes of GDP signals. The results indicate that GDP measurements are more sensitive to negative aggregate news than to positive aggregate news.

Finally, I investigate whether accounting conservatism's impact on macroeconomic indicators affects macroeconomic decision making. Specifically, I examine monetary policy decisions made by the U.S. Federal Reserve (the Fed). One way the Fed executes monetary policy is by manipulating the federal funds rate in order to influence the money supply and alter macroeconomic growth. The Fed relies on GDP measurements when setting the federal funds rate (Taylor 1993). Therefore, firm-level accounting conservatism may influence federal funds rate decisions by altering the GDP measurements upon which the Fed relies.

I investigate accounting conservatism's influence on Fed decisions by first quantifying the dollar value impact of conservatism on measurements of macroeconomic fundamentals. From 1955 to 2008, I estimate that aggregate corporate profits and GDP would have averaged approximately $30 billion greater per year if aggregate corporate profits incorporated good news in as timely a fashion as bad news. The dollar value impact of conservatism varies significantly over the sample period, and the amounts are economically significant, with the estimated downward influence averaging 0.4 percent of GDP. Finally, I show that incorporating the dollar value impact of conservatism increases the explanatory power of a monetary policy reaction function that describes U.S. Federal Reserve interest rate decision policy.

Prior research in accounting typically focuses on microeconomic behavior, and generally presumes that capital allocation decisions have been delegated to a market. For example, prior studies show that accounting provides new information to equity investors (Kothari 2001), facilitates efficient contracting (Watts and Zimmerman 1986; Ball 2001), disciplines managers' disclosure behavior (Gigler and Hemmer 1998; Stocken 2000), and improves managerial decision making (Waymire 2009). These prior results are important because they imply that accounting helps solve society's fundamental economic problem of maximizing social welfare by allocating scarce resources to their most efficient uses.

My study contributes to the literature by empirically identifying previously unexplored macroeconomic consequences of accounting. Instead of being a pure market economy, the U.S. is a mixed economy with elements of central planning, including an active central bank, partial nationalization of major banks and heavy industry, and fiscal intervention in times of financial crises. My results suggest that firm-level accounting conservatism has the potential to influence interest rates by altering the measurement attributes of key macroeconomic indicators used by the Fed. This suggests that accounting can affect the money supply, which influences not only resource allocation decisions, but also firms' investment opportunity sets, aggregate output and inflation, and total social welfare.

My results could also be of interest to policy setters. The Financial Accounting Standards Board's (FASB 2010) conceptual framework states that the aim of financial reporting is to provide information that is useful to investors and creditors in making decisions about providing resources to a firm. Although the FASB excludes regulators, fiscal policy setters, and other macroeconomic decision makers from the list of primary financial statement users (FASB 2010), financial reporting choices made by self-interested firms acting within the bounds of accounting standards can aggregate and influence output from the national economic accounts.

More specifically, the BEA uses financial accounting data to construct aggregate profit and GDP measures. Changes in the measurement of firm-level earnings, as defined by Generally Accepted Accounting Principles (GAAP), may alter the measurement of aggregate indicators published by the BEA. For example, the increased use of fair value measurements or the proposed convergence with International Financial Reporting Standards (IFRS) could indirectly change the measurement attributes of aggregate corporate profits and GDP. As a result, economic policy setters, such as central bankers, may improve their decision making by better understanding the impact of firm-level accounting practices on the national economic accounts.

Next, Section II motivates the empirical tests and reviews the literature. Section III constructs a proxy for aggregate news. Section IV examines whether aggregate corporate profits and GDP exhibit conditional conservatism. Section V quantifies the dollar value impact of conservatism. Section VI investigates whether the dollar value impact of conservatism influences monetary policy decisions, and Section VII concludes.

II. MOTIVATION AND PRIOR LITERATURE

Motivation

Whether firm-level conservatism aggregates to affect the measurement of macroeconomic indicators and influence monetary policy decisions are empirical questions. The summation of individual firm earnings could result in a conservative aggregate signal given that the demand for conservatism varies according to institutional factors that are systematic across firms (Ball, Kothari, and Robin 2000). For example, Seetharaman, Srinidhi, and Swanson (2005) demonstrate that conservatism declined after passage of the Private Securities Litigation Reform Act of 1995. The authors conclude that a decrease in litigation risk reduced firms' need for conservative reporting. Similarly, Lobo and Zhou (2006) find that conservatism increased after passage of the Sarbanes-Oxley Act of 2002,

consistent with an increase in litigation risk for managers and auditors. These results indicate that shocks to the regulatory and litigation environments can affect the degree of earnings conservatism for many firms simultaneously. Hence, a portion of the conservatism present in firms' earnings is likely attributable to systematic factors and, therefore, not diversified away upon aggregation.

Alternatively, aggregate corporate profits may fail to exhibit conditional conservatism for several reasons. First, aggregate corporate profits, as compiled by the BEA, include the earnings of both public and private firms. Private firms constitute the majority of all U.S. firms (Davis et al. 2006) and private firms have less incentive to report conservatively (Ball and Shivakumar 2005). Thus, aggregate corporate profits could fail to exhibit conditional conservatism given the relative mix and reporting incentives of the public and private firms in the population.

Second, Givoly, Hayn, and Natarajan (2007) employ simulation techniques and show that the use of aggregated data biases against detecting conditional conservatism. Additionally, I use measures of news and earnings aggregated across firms, i.e., not just across time for individual firms as in Givoly et al. (2007). This additional level of aggregation could further obscure conservatism at the aggregate level even if individual firms are reporting conservatively. For example, bad news for some firms can be good news for other firms due to competition within an industry or due to differing firm sensitivities to macroeconomic conditions (Shivakumar 2007).

Finally, accounting conservatism within aggregate corporate profits might be reduced or eliminated as a result of the BEA's source data and construction methods. For example, the BEA uses tax return data in addition to financial reporting data when constructing its measure of aggregate corporate profits.[1] Firms have less flexibility to make income-decreasing accruals for tax reporting as compared to financial reporting (Ball and Shivakumar 2005). This reduced flexibility may limit the degree of conditional conservatism within the BEA measure of aggregate corporate profits. Furthermore, the BEA's aggregation methodology involves replacing certain historical cost measures with current cost estimates. These adjustments could also reduce the degree of conservatism present (e.g., conditionally conservative lower-of-cost-or-market inventory write-downs are removed from BEA estimates of aggregate corporate profits).[2]

Prior Literature

My study is most closely related to the growing literature examining the properties of aggregate earnings. Anilowski, Feng, and Skinner (2007) find that changes in the aggregate proportion of upward management earnings guidance are associated with measures of aggregate earnings news. G. Sadka and R. Sadka (2009) find that prices better anticipate earnings growth at the aggregate level, and Cready and Gurun (2010) document a negative short-window relation between aggregate earnings surprises and aggregate returns. Kothari, Lewellen, and Warner (2006) fail to identify post-earnings announcement drift at the aggregate level, and Hirshleifer, Hou, and Teoh (2009) find that the accrual anomaly actually reverses at the aggregate level. Jorgensen, Li, and Sadka (2009) find that certain firm-level earnings attributes disappear in the aggregate (e.g., aggregate earnings fail to predict future aggregate cash flows) and that the informativeness of earnings to a diversified investor is largely unaffected by changes in accounting standards and enforcement.

I contribute to this literature by not only examining how firm-level accounting conservatism influences the measurement attributes of macroeconomic indicators, but also by examining how

[1] The BEA uses both data sources because neither source is individually sufficient to produce a timely summary measure of profits for all firms (BEA 2002; Himmelberg, Mahoney, Bang, and Chernoff 2004). For example, Internal Revenue Service (IRS) tax data cover both public and private firms, but tax returns are only available annually with a lag. In contrast, GAAP data are available for only public firms, but the data are available quarterly.

[2] See BEA (2002) for a more complete description of the differences in accounting methods between GAAP, the Internal Revenue Code, and the National Income and Product Accounts.

conservatism affects economic policy decisions, including interest rates set by the Federal Reserve. Additionally, I use BEA data that aggregate earnings for all firms, whereas Compustat only tracks the 1 percent of firms that are publicly traded. Also, BEA data are available beginning in 1929, versus 1962 for Compustat, thus increasing the power of my empirical tests.

My study also contributes to the accounting conservatism literature. Critics argue that conservatism can introduce a downward bias in income and net assets (FASB 2010). However, advocates of conservatism argue that if conservatism arises endogenously as part of a firm's solution to its profit maximization problem, then eliminating conservatism would constrain the firm and lower shareholder welfare (Watts 2003a). My study suggests that evaluations of the net benefit of conservatism should consider the macroeconomic consequences of conservatism, in addition to the impact on individual firms and stakeholders.

III. PROXY FOR AGGREGATE NEWS

In order to examine whether aggregate corporate profits and GDP are more sensitive to negative news than to positive news (i.e., whether the macroeconomic indicators are conditionally conservative), I require a proxy for aggregate news. I create a proxy for aggregate news using the return decomposition framework introduced by Campbell (1991). The logic behind the Campbell (1991) framework is that under rational expectations, unexpected aggregate stock returns must be related to changes in investors' expectations about aggregate future cash flows and discount rates. In other words, an unexpectedly high (low) current-period return implies that investors raised (lowered) their expectations about future cash flows, lowered (raised) their expectations about future discount rates, or both during the period. Because revisions in investors' expectations are not directly observable, Campbell (1991) forms empirical proxies using a first-order vector autoregression (VAR) of the form:

$$z_t = a + \Gamma z_{t-1} + u_t, \tag{1}$$

where z_t is an $m \times 1$ vector of macroeconomic state variables observable by the end of period t; a is an $m \times 1$ vector of constants; Γ is an $m \times m$ matrix of coefficient estimates; and u_t is an $m \times 1$ vector of independent and identically distributed residuals.

I implement Campbell's (1991) methodology by including four monthly variables in the VAR. First, I include the excess of the monthly return on the Center for Research in Securities Prices (CRSP) value-weighted index over the risk-free rate (*XRET*). Returns and risk-free rates used to calculate *XRET* are from Wharton Research Data Services (WRDS). The remaining three monthly variables included in the VAR are discount rate proxies. I include the difference between the yields on BAA- and AAA-rated corporate bonds (*DEF*) because prior research finds that this spread predicts future aggregate returns (Fama and French 1989). Corporate bond yields used to compute *DEF* are from the St. Louis Federal Reserve Economic Database. Next, I include the log of 1 plus the trailing 12-month dividend yield on the S&P 500 index (*DP*) because high dividends in relation to stock prices implies high discount rates (Fama and French 1989).[3] Finally, I include the

[3] The numerator of *DP* denotes the sum of reported quarterly dividends per share for all S&P 500 firms (regardless of fiscal year-end) over the past 12 months as of the most recent calendar quarter ending at least two months prior to the end of month t. For example, for the month ending October 31, 2000, the numerator would represent the sum of quarterly dividends per share for all S&P 500 firms over the 12 months ending June 30, 2000 (the more recent calendar quarter-end of September 30, 2000 did not occur at least two months before October 31, 2000). Constructing the numerator in this fashion is notable for two reasons. First, the use of quarterly data avoids any "look-ahead" bias induced by monthly interpolation. Second, the two-month lag ensures that firms' quarterly dividends have been publicly disclosed (the Securities and Exchange Commission's [SEC] quarterly filing requirement varies between 45 and 60 days over my sample period). The denominator of *DP* denotes the price level of the S&P 500 index at the end of month t from WRDS. S&P 500 dividend data are available on Professor Robert Shiller's website at: http://www.econ.yale.edu/~shiller/data.htm

TABLE 1

Proxy for Aggregate News

Panel A: Descriptive Statistics

Variable	n	Mean	Std. Dev.	Q1	Median	Q3	Augmented Dickey-Fuller
XRET	990	0.006	0.055	−0.021	0.009	0.036	−21.44***
DEF	990	0.011	0.007	0.007	0.009	0.013	−3.69***
DP	990	0.039	0.017	0.029	0.036	0.048	−3.77***
VALUE	990	1.634	0.362	1.403	1.509	1.718	−2.24

Panel B: Vector Autoregression Parameter Estimates

	Intercept	$XRET_{t-1}$	DEF_{t-1}	DP_{t-1}	$VALUE_{t-1}$	n	R^2	Durbin-Watson
$XRET_t$	0.004	0.125***	0.302	0.302**	−0.009	989	0.02	2.00
	(0.49)	(3.95)	(0.87)	(2.45)	(−1.45)			
DEF_t	0.000	−0.012***	0.966***	0.004	0.000*	989	0.96	1.81
	(−0.46)	(−14.13)	(105.08)	(1.11)	(1.70)			
DP_t	0.000	−0.007***	−0.054**	0.984***	0.001**	989	0.96	1.83
	(−0.37)	(−3.54)	(−2.50)	(128.65)	(2.38)			
$VALUE_t$	0.023***	0.011	0.633*	−0.104	0.984***	989	0.98	1.98
	(2.75)	(0.39)	(1.94)	(−0.91)	(175.02)			

***, **, * Represent two-tailed significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
This table presents descriptive statistics and parameter estimates for the first-order vector autoregression (VAR) system used to construct the *News* proxy for aggregate news. The sample period begins in July 1926 and ends in December 2008. Panel A presents descriptive statistics for the monthly variables. The augmented Dickey-Fuller statistic tests the null hypothesis of a unit root against the alternative hypothesis that the data series is stationary. Panel B presents parameter estimates for the VAR system. Each set of two rows corresponds to a single dependent variable within the VAR system. The first row of each set presents parameter estimates, and the second row presents t-statistics in parentheses. The columns present coefficient estimates for the independent variables, as well as the number of observations, the R^2, and the Durbin-Watson statistic for detecting autocorrelation in the residuals. The variables included in the VAR system are as follows.

Variable Definitions:
XRET = excess of the monthly return on the Center for Research in Securities Prices value-weighted index over the risk-free rate;
DEF = default spread, defined as the difference between the yield on a portfolio of seasoned BAA corporate bonds and the yield on seasoned AAA corporate bonds;
DP = log of 1 plus the trailing 12-month dividend yield on the S&P 500 index; and
VALUE = small stock value spread, defined as the difference between the logs of the book-to-market ratios of small value stocks and small growth stocks.

difference between the log book-to-market ratios of small value stocks and small growth stocks (*VALUE*) because small growth stocks are more dependent on external financing and are more sensitive to discount rate shocks (Campbell and Vuolteenaho 2004). Book-to-market ratio and return data for six portfolios formed on size and book-to-market used to construct *VALUE* are available on Professor Kenneth French's website at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html/.

Table 1 presents descriptive statistics and parameter estimates for the vector autoregression system. Within Panel A, augmented Dickey-Fuller (ADF) test statistics show that the *XRET*, *DEF*,

and *DP* variables are stationary. While I cannot reject the null hypothesis of a unit root for the *VALUE* variable, a Durbin-Watson test within the aggregate return equation in Panel B shows that there is no autocorrelation (either positive or negative) in the residuals. Within Panel B, the positive and significant coefficient on $XRET_{t-1}$ of 0.125 is consistent with momentum within aggregate returns. The positive and significant coefficient on DP_{t-1} of 0.302 is consistent with prior research showing that larger aggregate dividend-to-price ratios predict higher aggregate returns (Fama and French 1989). The coefficient on DEF_{t-1}, while not significant at conventional levels, is directionally consistent with prior studies that find a positive correlation between the default spread and future aggregate returns (Fama and French 1989). Finally, the adjusted R^2 of 2 percent for the aggregate returns and the remaining VAR results are all generally consistent with prior research (see Campbell and Vuolteenaho 2004).

The intuition is that the VAR in Model (1) decomposes aggregate returns into an expected component and an unexpected component. The expected component represents risk, and the unexpected component (i.e., the residual) denotes news. Summing the resulting monthly aggregate return residuals by year yields my annual proxy for aggregate news (*News*). Conceptually, *News* represents the summation of changes in investors' expectations about aggregate future cash flows and aggregate future discount rates during the year.[4]

Figure 1 plots the time series of *News*. Positive (negative) values denote good (bad) news. *News* exhibits significant time-series variation and tends to move with the business cycle. For example, *News* is generally negative during recessions (although "bad news" observations are not limited to recessions or anomalies like the Great Depression).

Descriptive statistics for *News* can be found in Table 2. The mean of *News* is indistinguishable from 0 (as desired), and augmented Dickey-Fuller tests suggest that *News* is stationary. Overall, the descriptive evidence suggests that *News* approximates a symmetrically distributed random variable with a mean of 0.

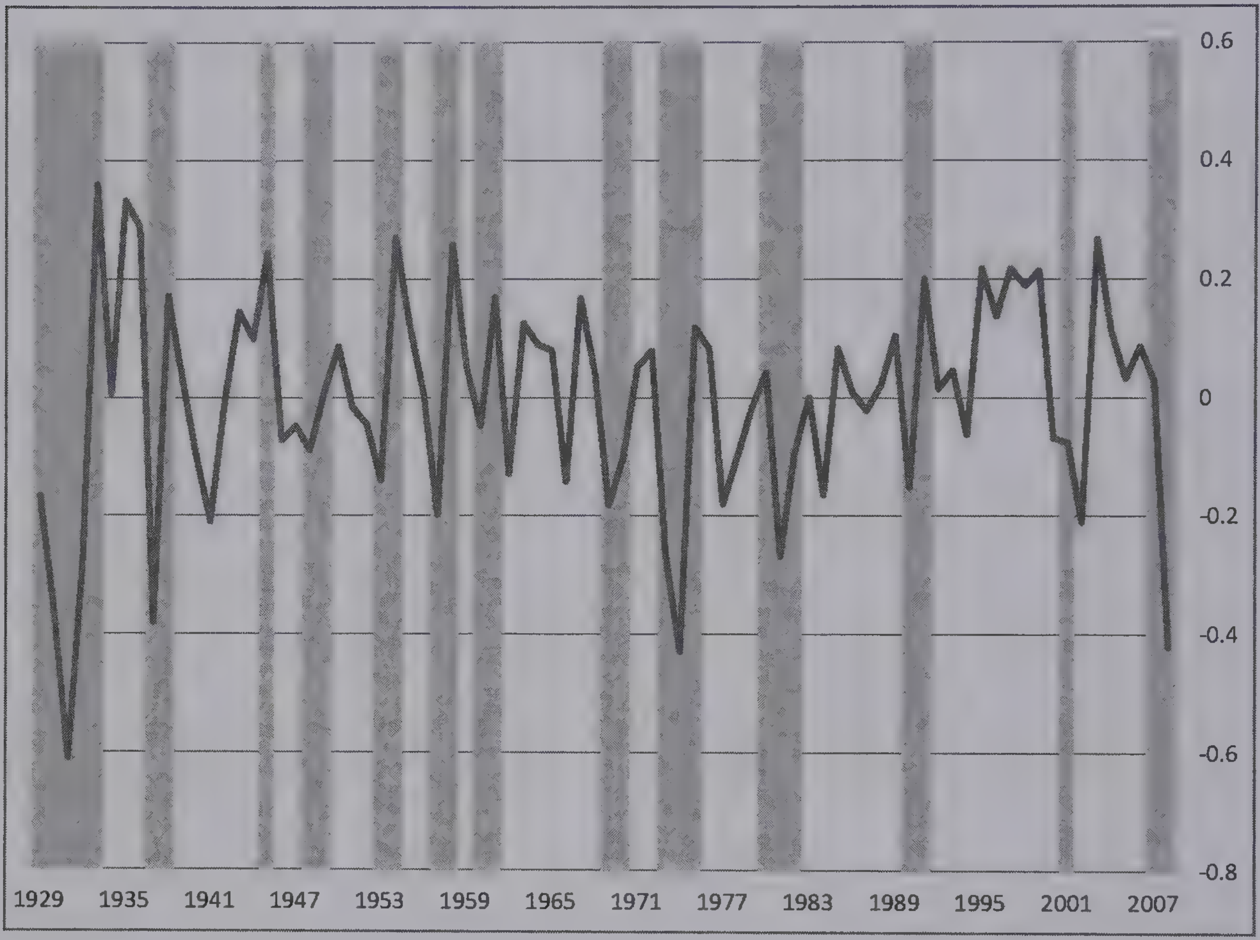
Robustness Checks

I perform a variety of (untabulated) robustness checks regarding my aggregate news proxy. First, I explore the use of alternate variables in the vector autoregression. I include the aggregate dividend-to-price ratio (*DP*) in Model (1) above because Engsted, Pedersen, and Tanggaard (2012) show that *DP* must be included in order for the assumptions of the Campbell (1991) return decomposition to be valid. However, other studies in the literature (e.g., Campbell and Vuolteenaho 2004) use the price-to-earnings ratio on the S&P 500 (*PE*) in lieu of *DP*. The correlation between *DP* and *PE* in my sample is –0.74 ($p < 0.01$), and all results are quantitatively and qualitatively similar when using *PE* instead of *DP*.

Second, I further investigate whether *News* is a well-specified exogenous shock. One potential concern is that the split between negative and positive aggregate news may depend on aggregate firm characteristics (e.g., perhaps *News* is more likely to be negative in periods when the corporate sector as a whole is more levered). Callen and Segal (2013) account for this potential endogeneity when measuring conservatism at the firm level using a switching regression approach. I explore the need for a switching approach by constructing annual aggregate estimates of the determinants of conservatism shown to be significant in Callen and Segal (2013). In (untabulated) univariate and multivariate analyses, I find no evidence that *News* is correlated with aggregate information

[4] For an example of the Campbell (1991) approach at the aggregate level in the macroeconomics literature, see Bernanke and Kuttner (2005). Accounting and finance researchers have also adapted the Campbell (1991) framework to the firm level. See Callen, Segal, and Hope (2010), Vuolteenaho (2002), Callen and Segal (2004, 2013), Callen, Hope, and Segal (2005), Callen, Livnat, and Segal (2006), and Sadka (2007).

FIGURE 1
Proxy for Aggregate News



This figure plots the *News* proxy for aggregate news. $News_t$ denotes the sum of monthly cash flow and discount rate shocks in year t formed from the first-order vector autoregression system presented in Table 1. See Section III for discussion. The sample period begins in 1929 and ends in 2008. Shaded regions denote recessions as defined by the National Bureau of Economic Research.

asymmetry, leverage, litigation risk, or taxes. Thus, *News* appears to be a well-specified exogenous shock.[5]

IV. ACCOUNTING CONSERVATISM AND MACROECONOMIC INDICATORS

This section investigates whether measurements of aggregate corporate profits and GDP are more sensitive to negative aggregate news than to positive aggregate news (i.e., whether the macroeconomic indicators are conditionally conservative).[6] I begin by investigating whether the

[5] The lack of correlation between aggregate news and aggregated firm characteristics is not surprising given that the VAR in Model (1) is designed to produce a measure of aggregate news that is orthogonal with respect to other systematic macro factors (e.g., the default spread, the aggregate dividend yield, and the small stock value premium). Given these results, I do not employ a switching regression approach in Section IV because doing so would reduce my sample by 37 percent (requiring Compustat data would eliminate the years 1929–1961 from my sample).

[6] My empirical tests focus on conditional conservatism rather than unconditional conservatism for two primary reasons. First, the national economic accounting system does not produce an aggregate corporate-sector analog to an individual firm's balance sheet. Second, measurements of the combined market value of the entire U.S. corporate sector (i.e., both public and private firms) are unavailable. Hence, examining unconditional conservatism using traditional market-to-book measures is extremely difficult at the macroeconomic level.

TABLE 2

Descriptive Statistics for Aggregate-Level Data

Variable	n	Mean	Std. Dev.	Q1	Median	Q3	Augmented Dickey-Fuller
CP/FAPrivate	80	0.04	0.02	0.04	0.05	0.05	−3.47**
GDP/FATotal	80	0.37	0.04	0.35	0.37	0.39	−3.95***
News	80	0.00	0.19	−0.10	0.01	0.12	−6.34***
Neg	80	0.44	0.50	0.00	0.00	1.00	−7.64***

***, **, * Represent two-tailed significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
This table presents descriptive statistics for select aggregate annual time-series variables. The sample period begins in 1929 and ends in 2008. The augmented Dickey-Fuller statistic tests the null hypothesis of a unit root against the alternative hypothesis that the data series is stationary.

Variable Definitions:
CP = aggregate corporate profits in year *t* from Line 17 of the Bureau of Economic Analysis (BEA) National Income and Product Account (NIPA) Table 1.7.5;
GDP = gross domestic product in year *t* from Line 1 of BEA NIPA Table 1.7.5;
FATotal = current-cost value of net fixed assets owned by public and private businesses, nonprofit institutions, and governments in year *t*–1 from Line 2 of BEA Fixed Asset Table 1.1;
FAPrivate = current-cost value of net fixed assets owned by public and private businesses in year *t*–1 from Line 3 of BEA Fixed Asset Table 1.1;
News = sum of monthly cash flow and discount rate shocks in year *t* formed from the first-order vector autoregression system presented in Table 1; and
Neg = a dummy variable that equals 1 if *News* < 0, and equals 0 otherwise.

time-series of aggregate corporate profits exhibits conditional conservatism. Specifically, I adapt Basu's (1997) asymmetric timeliness framework to accommodate aggregate-level time-series data with the goal of estimating the following regression:

$$CP_t/FAPrivate_{t-1} = \theta_0 + \theta_1 News_t + \theta_2 Neg_t + \theta_3 News_t{*}Neg_t + \mu_t, \tag{2}$$

where CP_t denotes aggregate corporate profits from Line 17 of BEA National Income and Product Account (NIPA) Table 1.7.5.[7] $FAPrivate_{t-1}$ is the current-cost value of net fixed assets owned by all businesses in year $t-1$ from Line 3 of BEA Fixed Asset Table 1.1.

Econometric Issues

While the approach above for detecting conditional conservatism is intuitively appealing, prior research has identified econometric concerns with the Basu (1997) approach at the firm level. These concerns may also apply to the aggregate specification in Model (2). First, Dietrich, Muller, and Riedl (2007) and Beaver, Landsman, and Owens (2012) find that t-statistics can be misspecified at the firm level due to endogeneity within the earnings-return relation (i.e., the release of a firm's earnings might influence returns for the firm). However, the macroeconomics literature fails to find any abnormal stock market activity (returns or volume) during the short window around the release of many macroeconomic indicators, including GDP (Flannery and Protopapadakis 2002). Additionally, with respect to aggregate corporate profits, investors should have impounded any

[7] Aggregate corporate profits represent the pre-tax sum of profits from current production earned by all entities required to file a federal tax return (BEA 2002). The BEA estimate for calendar year *t* is released in July of year *t*+1 and revised in years *t*+2 and *t*+3. I use the latest available estimates for all empirical tests.

new information contained within public firms' earnings announcements for year t before the release of the aggregate signal by the BEA in year $t+1$. In other words, the information provided by the release of the aggregate corporate profits signal is at least partially redundant given the previous earnings announcements of individual firms. Hence, reverse causality should be less of a concern at the aggregate level than at the firm level.[8]

A second econometric concern with the Basu (1997) approach is the potential for biased coefficient estimates arising from splitting the sample based on the sign of the news variable (Maddala and Lahiri 2009; Dietrich et al. 2007). As a solution, I estimate reverse truncated regressions separately for good news and bad news subsamples. The subsample regressions are estimated using maximum likelihood techniques that control for the potential parameter bias (see Hausman and Wise 1977; Maddala 1983). I then use the subsample results to construct piecewise regression output in the form of Model (2) with bias-corrected parameters.

Results

Table 2 presents descriptive statistics for the variables in Model (2). Aggregate corporate profits and aggregate fixed assets are measured in billions of nominal year t dollars. The scaled aggregate corporate profits variable used for regression purposes is stationary.[9] The mean scaled aggregate corporate profits value of 0.04 indicates that aggregate return on fixed assets for the entire corporate sector is approximately 4 percent.

Although all of the variables in Model (2) are stationary and truncation bias has been accounted for, the parameter estimates may be significantly influenced by the presence of outliers due to the small sample size.[10] I identify outliers using studentized residuals and the change in each parameter estimate after deleting the ith observation following Belsley, Kuh, and Welsch (1980). This procedure highlights three years (1932–1934) as outliers. These observations represent the later years of the Great Depression. The period from 1932–1934 includes the only years in the sample where aggregate corporate profits are negative. These realizations are quite extreme (i.e., the sum of the earnings for all firms in the entire country resulted in a net loss). However, Figure 1 shows that the *News* realizations during the same period are among the most positive in the sample. As a result, the inclusion of these observations significantly influences the θ_1 coefficient downward while influencing the θ_3 coefficient upward (i.e., including the outliers actually biases for finding conditional conservatism at the aggregate level). Because these observations are outliers in both a statistical sense and an economic sense (i.e., aggregate corporate profits during the Great Depression may be viewed as coming from a different data-generating process), I exclude these observations from the sample for regression purposes.

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[8] See also Ball, Kothari, and Nikolaev (2013a, 2013b) for an econometric defense of the Basu (1997) approach. Additionally, Ball and Shivakumar (2008) find that information in earnings announcements can explain only a small fraction of firm-level returns, suggesting that reverse causality is not a first-order concern even at the firm level. See also Ryan (2006), who asserts that asymmetric timeliness is still the most direct implication of conditional conservatism despite the potential limitations.

[9] Generating a stationary variable by scaling is advantageous compared to using a real (i.e., inflation-adjusted) measure of aggregate corporate profits because real aggregate corporate profits are non-stationary. That is, even in the absence of inflation, aggregate corporate profits (and GDP) tend to increase due to technological progress. Using lagged fixed assets as a scalar could also mitigate artificial overstatement of conservatism arising from the use of lagged price as a scalar (Patatoukas and Thomas 2011).

[10] Despite the small sample size, my empirical tests employ annual estimates (rather than quarterly estimates) for three reasons. First, certain source data are only available on an annual basis, and the BEA must extrapolate and make seasonal adjustments to construct quarterly estimates (BEA 2008). Second, the source data underlying quarterly estimates are not as reliable as the source data underlying annual estimates (BEA 2008). Finally, annual data are available beginning in 1929, while quarterly data are only available beginning in 1946 (BEA 2002).

TABLE 3

Sensitivity of Aggregate Corporate Profits and GDP to Aggregate News

$$Dependent\ Variable = \theta_0 + \theta_1 News_t + \theta_2 Neg_t + \theta_3 News_t{*}Neg_t + \mu_t.$$

Model (2)			Model (3)		
Dependent Variable: $CP_t/FAPrivate_{t-1}$			**Dependent Variable: $GDP_t/FATotal_{t-1}$**		
Variable	**Coefficient**	**t-statistic**	**Variable**	**Coefficient**	**t-statistic**
Intercept	0.046	(21.70)***	Intercept	0.371	(59.15)***
News	0.002	(0.28)	*News*	0.003	(0.17)
Neg	0.003	(0.85)	*Neg*	0.010	(1.18)
*News * Neg*	0.032	(2.17)**	*News * Neg*	0.090	(2.45)**
n		77	n		77
R^2		0.129	R^2		0.122

***, **, * Represent two-tailed significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
This table reports the results from separate time-series regressions of scaled aggregate corporate profits and gross domestic product on aggregate news. The regressions were estimated using maximum likelihood techniques that control for parameter bias arising from splitting the sample based on the sign of the news. The sample period begins in 1929 and ends in 2008. The Great Depression years 1932–1934 were identified as outliers and are excluded for regression purposes. See Section IV for further discussion. t-statistics are presented in parentheses.
See Table 2 for variable definitions.

Table 3 presents the piecewise regression results (with bias-corrected parameters) for Model (2) designed to determine whether aggregate corporate profits exhibit conditional conservatism. The positive θ_1 coefficient of 0.002 suggests that scaled aggregate corporate profits are higher during periods of more positive aggregate news (as expected). However, the θ_1 coefficient is not statistically significant (in contrast to prior firm-level results), potentially due to a lack of power.[11] Because the θ_1 coefficient is indistinguishable from zero, the $(\theta_1 + \theta_3)/\theta_1$ expression commonly used to provide context in firm-level studies is undefined in my sample. As such, I am unable to make comparisons such as "earnings are X times more sensitive to bad news than to good news." However, the sum of the θ_1 and θ_3 coefficients (i.e., the numerator in the expression above) is significantly greater than zero at the 5 percent level. Turning to the interaction term, the θ_3 coefficient of 0.032 is significantly positive. This suggests that aggregate corporate profits are more sensitive to negative aggregate news than to positive aggregate news, consistent with the existence of conditional conservatism at the aggregate level.

Identifying conditional conservatism within aggregate corporate profits is important because aggregate corporate profits are a significant component of GDP.[12] As such, I next examine whether conservatism's effect extends beyond aggregate corporate profits to alter the measurement attributes

[11] Firm-level conservatism studies typically utilize large panel datasets. In contrast, my study has only 77 observations for one macroeconomic unit (i.e., the U.S. economy). Such short time-series are typical within the empirical macroeconomics literature, and I likely have less statistical power compared to firm-level accounting studies. Additionally, recent firm-level conservatism studies (e.g., LaFond and Watts 2008) generate a positive and significant main effect coefficient by estimating Fama and MacBeth (1973) regressions by year. However, such cross-sectional econometric treatments are not possible with the pure time-series data in my study.

[12] GDP represents the market value of final goods and services produced in the U.S. (BEA 2008). The BEA measures GDP in multiple ways. First, the BEA uses an "expenditures approach" to arrive directly at a GDP estimate. Second, the BEA measures the income derived from the sale of final goods and services. This "income approach" utilizes aggregate corporate profits as a direct input and yields an estimate of gross domestic income (GDI). The univariate correlation between GDP and GDI exceeds 0.99 in my sample. The results of all empirical tests utilizing GDP estimates are quantitatively and qualitatively similar when using GDI.

of GDP. Specifically, I investigate whether GDP exhibits conditional conservatism by constructing the following piecewise regression with bias-corrected parameters:

$$GDP_t/FATotal_{t-1} = \Phi_0 + \Phi_1 News_t + \Phi_2 Neg_t + \Phi_3 News_t{*}Neg_t + \omega_t, \tag{3}$$

where GDP_t denotes gross domestic product in year t from Line 1 of BEA NIPA Table 1.7.5.[13] $FATotal_{t-1}$ represents the current-cost value of net fixed assets owned by all businesses, nonprofit institutions, and governments in year $t-1$ from Line 2 of BEA Fixed Asset Table 1.1.

Table 2 presents descriptive statistics for the variables in Model (3). GDP is measured in billions of nominal year t dollars. An augmented Dickey-Fuller test shows that the scaled GDP variable used for regression purposes is stationary. The mean and median value for scaled GDP of 0.37 indicates that the U.S. economy produces goods and services with a final market value of \$0.37 for each dollar of assets owned by businesses, nonprofit institutions, and governments.

Table 3 presents the piecewise regression results (with bias-corrected parameters) for Model (3) designed to determine whether GDP exhibits conditional conservatism. The results from Model (3) are very similar to the aggregate corporate profit results from Model (2). Specifically, the Φ_3 coefficient of 0.090 is positive and significant. These results suggest that GDP estimates are more sensitive to negative aggregate news than to positive aggregate news, consistent with the existence of conditional conservatism at the aggregate level.

Robustness Checks

I perform a variety of (untabulated) robustness checks for the regressions presented in Table 3. First, I consider alternate proxies for aggregate news. For example, I further decompose the *News* variable into aggregate discount rate news and aggregate cash flow news components following Campbell (1991). Using both aggregate corporate profits and GDP, the interaction term on the aggregate cash flow news variable is positive and significant when included alone or in conjunction with the aggregate discount rate news variables. However, none of the coefficients on the aggregate discount rate variables are significant. These results suggest that the asymmetric sensitivity of aggregate corporate profits and GDP to aggregate news is driven by the cash flow news component of aggregate news.[14] Additionally, all results using aggregate cash flow news are quantitatively and qualitatively similar using an alternative measure of aggregate cash flow news based on Chen and Zhao (2009).

Second, the standard errors from the truncated regressions used to construct Models (2) and (3) are not robust to the presence of autocorrelation in the residuals. However, all piecewise regression results are quantitatively and qualitatively similar when estimated using Newey and West (1987) adjusted standard errors.[15]

Third, I examine the impact of outliers in more detail. Currently, the only regression that uses the Great Depression years 1932–1934 is the vector autoregression in Table 1 (because vector autoregressions require an uninterrupted time-series). As a robustness check, I exclude the Great

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[13] The estimate of GDP for calendar year t is released in July of year $t+1$ and revised in years $t+2$ and $t+5$. I use the latest available estimates for all empirical tests.

[14] This result is not surprising for two reasons. First, the BEA uses GAAP data as an input, and the FASB designs GAAP to help predict the timing, magnitude, and uncertainty of firms' future cash flows (but not necessarily firms' future discount rates). Similarly, the BEA's National Income and Product Accounts are meant to reflect the size, composition, and uses of national income (as opposed to aggregate expected returns).

[15] Reestimating these models using Newey and West (1987) standard errors requires relaxing the constraint that the parameter estimates are equal to their bias-corrected values. In other words, the parameter estimates can be corrected for truncation bias, or Newey and West (1987) standard errors can be used to control for potential autocorrelation in the residuals. However, both corrections cannot be done simultaneously.

Depression years from the vector autoregression (which necessitates beginning the sample for the analyses in Tables 1 through 3 in 1935). The vector autoregression results in Table 1 and the aggregate regression results in Table 3 remain qualitatively similar to the main results. Additionally, I reestimate the aggregate regressions in Table 3 with the Great Depression years 1932–1934 included. The Φ_3 coefficient of interest remains positive and significant, although the Φ_1 coefficient becomes negative (but remains insignificant).

Fourth, I replicate Model (2) using aggregate Compustat earnings as the dependent variable in an attempt to show that the conservatism within aggregate corporate profits and GDP is a function of the conservatism within firm-level GAAP earnings. The resulting θ_3 coefficient is not statistically different from zero. Thus, similar to Jorgensen et al. (2009), I am unable to reject the null hypothesis of no conditional conservatism within aggregate Compustat earnings. However, Compustat data are only available beginning in 1962, whereas BEA data are available from 1929. In order to investigate a lack of power as an explanation for the insignificant coefficient of interest when using aggregate Compustat earnings, I reestimate Model (2) using BEA data over two subperiods: (1) the pre-Compustat sample period, and (2) the Compustat period. The results show an insignificant θ_3 coefficient for the BEA data in both subperiods. Because the θ_3 coefficient for the BEA data is positive and significant in the unrestricted sample, but insignificant in both reduced samples, the failure to identify conditional conservatism within aggregate Compustat earnings may be attributed to Compustat's limited sample period.

Finally, I attempt to rule out the BEA's use of aggregated tax data as the source of the conservatism within aggregate corporate profits and GDP. I replicate Model (2) using aggregate taxable income from Column 3 of the Internal Revenue Service's Statistics of Income Table 15 as the dependent variable. The results indicate a failure to reject the null hypothesis of no conditional conservatism within aggregate taxable income. This result is consistent with the conservatism within aggregate corporate profits and GDP being a function of the conservatism within firm-level GAAP earnings rather than a function of the BEA's use of tax source data. However, aggregated IRS data are only available back to 1960 and, thus, I cannot rule out insufficient power as an alternative explanation.

Summary

The results in this section show that measurements of aggregate corporate profits and gross domestic product compiled by the U.S. Bureau of Economic Analysis are more sensitive to negative aggregate news than to positive aggregate news. These results are consistent with firm-level accounting conservatism aggregating to influence the measurement attributes of key macroeconomic indicators. However, data limitations prevent me from definitively isolating the conservatism within firm-level GAAP earnings as the source of the conservatism within aggregate corporate profits and GDP. I also acknowledge that my empirical tests are constrained by the limits of aggregate-level data. For example, BEA data are not conducive to adapting alternative firm-level measures of conservatism that require accruals information, book-to-market data, or other detailed financial statement line items.[16]

V. THE DOLLAR VALUE IMPACT OF CONSERVATISM

The results in the previous section suggest that aggregate corporate profits and GDP are more sensitive to negative aggregate news than positive aggregate news. The aim of the remaining

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[16] See Penman and Zhang (2002), Roychowdhury and Watts (2007), Beaver and Ryan (2005), Givoly et al. (2007), Callen et al. (2010), Khan and Watts (2009), and Caskey and Peterson (2014).

empirical tests is to examine whether the presence of conditional conservatism at the aggregate level influences monetary policy decisions. In order to measure conservatism's influence on Fed decision making using standard macroeconomics techniques, I require an estimate of the dollar value impact of conservatism on aggregate corporate profits and GDP.

I estimate the dollar value impact of conservatism on aggregate corporate profits and GDP as follows. First, I use the estimated regression coefficients and the residuals from Model (2) in Table 3 to create an estimate of scaled aggregate corporate profits if scaled aggregate corporate profits incorporated good news in as timely a fashion as bad news:

$$CP_t^*/FAPrivate_{t-1} = \theta_0 + (\theta_1 + \theta_3)News_t + \theta_2 Neg_t + \mu_t. \tag{4}$$

Subtracting Model (2) from Model (4) and multiplying by $FAPrivate_{t-1}$ to remove the fixed asset scalar yields an estimate of the year t dollar value impact of conditional conservatism on measurements of aggregate corporate profits and GDP ($CONS_t$):

$$CONS_t = CP_t^* - CP_t = FAPrivate_{t-1} * \theta_3 * News_t * (1 - Neg_t). \tag{5}$$

$CONS_t$ is converted from nominal year t dollars into real (year-2005) dollars using the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9. Larger positive values denote a greater dollar value impact (e.g., a $CONS_t$ value of 100 indicates that aggregate corporate profits and GDP would have been approximately \$100 billion higher if aggregate corporate profits incorporated good news in as timely a fashion as bad news).

Descriptive statistics for the *CONS* variable are presented in Table 4. The mean *CONS* value of 30.40 suggests that aggregate corporate profits and GDP would have averaged approximately \$30 billion greater per year if aggregate corporate profits incorporated good news in as timely a fashion as bad news. This equates to an economically significant average downward influence on GDP of 0.4 percent per year from 1955 to 2008. An augmented Dickey-Fuller test statistic indicates that *CONS* is stationary.

See Figure 2 for a plot showing that *CONS* exhibits significant time-series variation. While *CONS* is stationary, the largest realizations occur later in the sample period for two reasons. First, Equation (5) shows that *CONS* is increasing in the magnitude of the *News* variable, and some of the highest *News* realizations occurred in the late 1990s and early 2000s (the period of high equity market returns during the "technology bubble"). Second, *CONS* is increasing in the magnitude of *FAPrivate*, and aggregate fixed assets increase over the sample period even after controlling for inflation. As such, I plot *CONS* as a percentage of GDP (see Figure 3). The percentage impact of *CONS* on GDP varies significantly, and the variation includes both increasing and decreasing trends. Thus, while *CONS* is only strictly positive in "good news" years by construction, and I do not estimate the cumulative effect of conditional conservatism (the BEA does not construct an aggregate balance sheet), the plot in Figure 3 is consistent with conservatism at the aggregate level both increasing and reversing over time.

Robustness Checks

One potential concern when examining whether the dollar value impact of conservatism on GDP influences monetary policy decisions is that underlying macroeconomic forces could be driving both *CONS* realizations and the federal funds rate. However, (untabulated) univariate and multivariate analyses show that *CONS* is not significantly correlated with several prominent leading and lagging economic indicators cited in the literature and the popular press, including residential housing starts, the unemployment rate, the percentage change in the Index of Leading Economic Indicators, and consumer sentiment.

TABLE 4

Descriptive Statistics for Monetary Policy Variables

Variable	n	Mean	Std. Dev.	Q1	Median	Q3	Augmented Dickey-Fuller
FEDFUNDS	54	5.64	3.40	2.98	5.22	7.31	−2.75*
GAP1	54	26.96	142.58	−57.88	26.17	101.20	−4.27***
GAP2	54	−3.44	151.49	−74.90	−7.12	52.77	−4.02***
INF	54	3.60	2.28	2.08	2.96	4.32	−2.20
CONS	54	30.40	47.13	0.00	10.47	38.15	−3.59***
VOLCKER	54	0.15	0.36	0.00	0.00	0.00	−2.06
GREENSPAN	54	0.33	0.48	0.00	0.00	1.00	−1.51
BERNANKE	54	0.06	0.23	0.00	0.00	0.00	−0.14

***, **, * Represent two-tailed significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
This table reports descriptive statistics for variables used in the monetary policy reaction functions. All variables denominated in dollars have been converted into real year-2005 dollars using the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9. The sample period begins in 1955 and ends in 2008. The augmented Dickey-Fuller statistic tests the null hypothesis of a unit root against the alternative hypothesis that the data series is stationary.

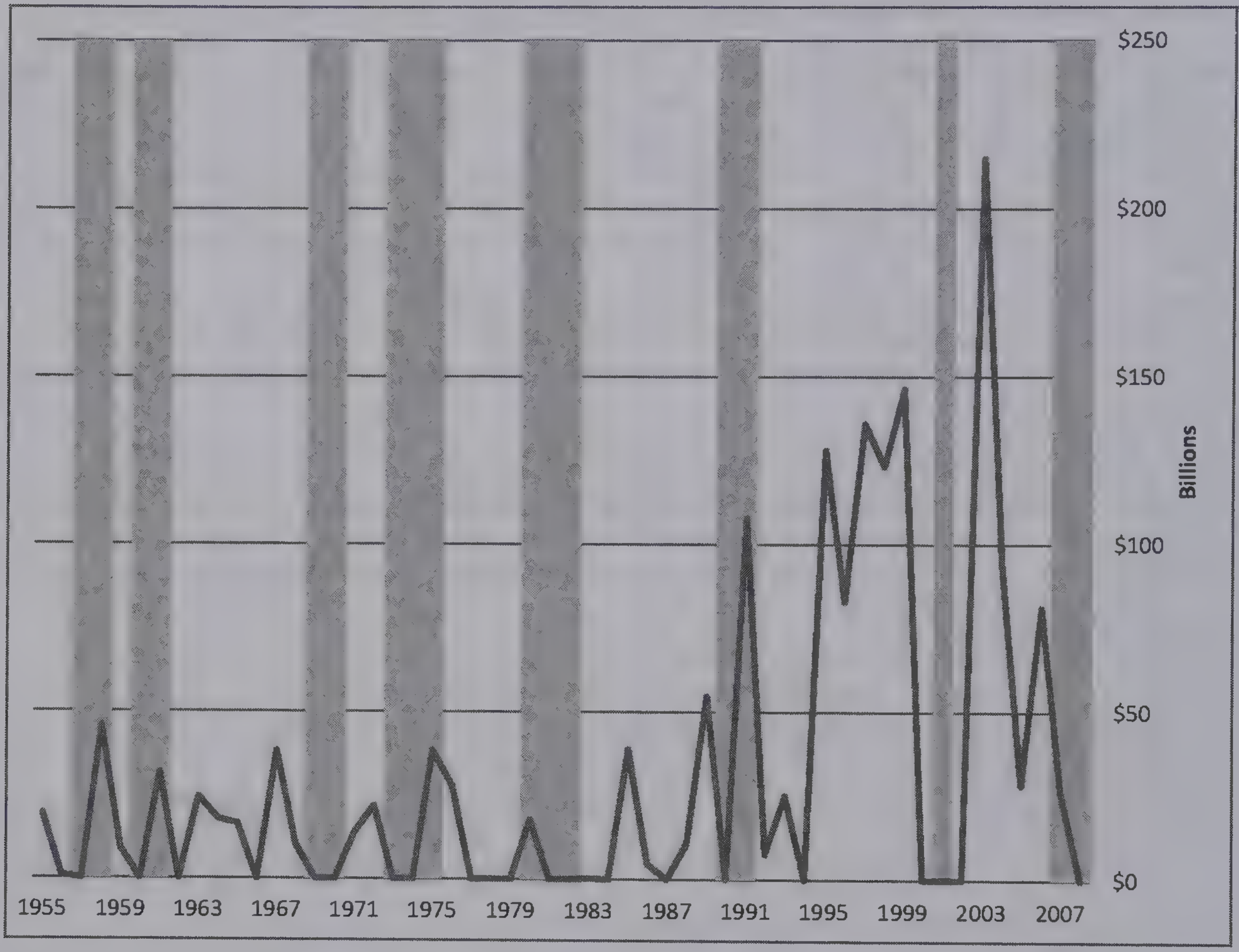
Variable Definitions:
FEDFUNDS = the nominal federal funds rate at the end of year t obtained from Federal Reserve Statistical Release H.15;
INF = inflation calculated as the percentage change in the implicit GDP deflator from Line 1 of the Bureau of Economic Analysis (BEA) National Income and Product Account (NIPA) Table 1.1.9 from year t–1 to year t;
VOLCKER (*GREENSPAN*) [*BERNANKE*] = dummy variables that equal 1 if Paul Volcker (Alan Greenspan) [Ben Bernanke] was the Chairman of the Federal Reserve in year t, and equal 0 otherwise;
CONS = an estimate of the dollar value impact of conservatism on aggregate corporate profits and gross domestic product. $CONS_t = FAPrivate_{t-1} * \Theta_3 * News_t * (1 - Neg_t)$, where $FAPrivate_{t-1}$ is the current-cost value of net fixed assets owned by public and private businesses in year t–1 from Line 3 of BEA Fixed Asset Table 1.1, Θ_3 is an estimated regression coefficient from Model (2) presented in Table 3 that reflects the incremental sensitivity of aggregate corporate profits to bad news as compared to good news, $News_t$ is the sum of monthly cash flow and discount rate shocks in year t formed from the first-order vector autoregression system presented in Table 1, and Neg_t is a dummy variable that equals 1 if $News_t < 0$, and equals 0 otherwise. Larger positive values denote a greater dollar value impact of accounting conservatism on GDP (e.g., a $CONS_t$ value of 100 indicates that real GDP in year t would have been approximately $100 billion higher if aggregate corporate profits incorporated good news in as timely a fashion as bad news). See Section V for discussion;
GAP1 = a measure of the output gap in year t, defined as $GDPPotential_t - GDP_t$;
GDPPotential = an estimate of potential GDP (i.e., gross domestic product if the economy was operating at full employment) in year t from the Congressional Budget Office;
GDP = gross domestic product for year t from Line 1 of BEA NIPA Table 1.7.5; and
GAP2 = a measure of the output gap in year t adjusted for the impact of accounting conservatism defined as $GDPPotential_t - (GDP_t + CONS_t)$.

VI. ACCOUNTING CONSERVATISM AND THE FEDERAL FUNDS RATE

This section investigates whether the dollar value impact of conservatism on GDP influences monetary policy decisions made by the Federal Reserve.[17] Taylor (1993) demonstrates that the Fed

[17] The Fed's mandate from the U.S. Congress under Section 2a of the Federal Reserve Act of 1913 is to "promote effectively the goals of maximum employment, stable prices, and moderate long-term interest rates" (U.S. House of Representatives 1913). One way the Fed influences both output and prices is by setting a target for the federal funds rate. The federal funds rate is the interest rate that banks charge one another for overnight loans. The Federal Open Market Committee raises (lowers) the federal funds target in order to make it more (less) expensive for banks to borrow from one another in order to meet minimum reserve requirements. In turn, the increased (decreased) cost to meet reserve requirements discourages (encourages) bank lending and ultimately exerts downward (upward) pressure on the money supply.

FIGURE 2
The Dollar Value Impact of Conservatism



This figure plots $CONS_t$, an estimate of the dollar value impact of conservatism on aggregate corporate profits and gross domestic product.

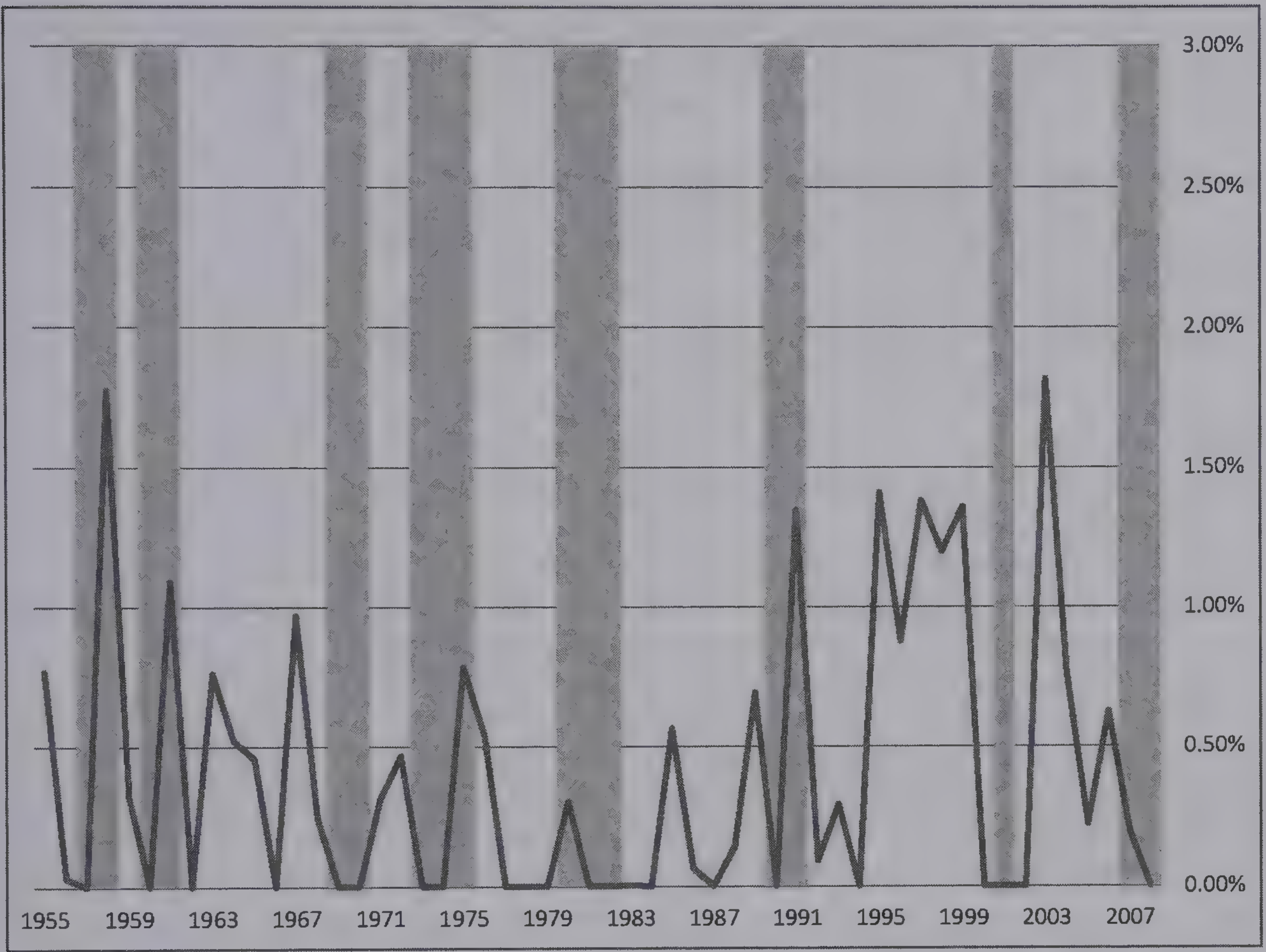
$CONS_t = FAPrivate_{t-1} * \Theta_3 * News_t * (1 - Neg_t)$, where $FAPrivate_{t-1}$ is the current-cost value of net fixed assets owned by public and private businesses in year $t-1$ from Line 3 of Bureau of Economic Analysis (BEA) Fixed Asset Table 1.1. Θ_3 is an estimated regression coefficient from Model (2) presented in Table 3 that reflects the incremental sensitivity of aggregate corporate profits to bad news as compared to good news, and $News_t$ is the sum of monthly cash flow and discount rate shocks in year t formed from the first-order vector autoregression system presented in Table 1. Neg_t is a dummy variable that equals 1 if $News_t < 0$, and Neg_t equals 0 otherwise. See Section V for discussion.

$CONS_t$ has been converted into real year-2005 dollars using the implicit GDP deflator from Line 1 of BEA National Income and Product Account Table 1.1.9. Larger positive values denote a greater dollar value impact of accounting conservatism on GDP (e.g., a $CONS_t$ value of 100 indicates that real GDP in year t would have been approximately \$100 billion higher if aggregate corporate profits incorporated good news in as timely a fashion as bad news).

The sample period begins in 1955 and ends in 2008. Shaded regions denote recessions as defined by the National Bureau of Economic Research.

utilizes GDP measurements when setting the federal funds rate. Taylor's (1993) results, combined with my results above, suggest that accounting conservatism may influence interest rate decisions by altering the GDP measurements upon which the Fed relies. For example, a central banker who observes a GDP signal in a good news period may be unsure as to whether true (unobserved) GDP is actually higher than the signal because aggregate corporate profits fail to incorporate good news in as timely a fashion as bad news.

FIGURE 3
The Percentage Impact of Conservatism



This figure plots $CONS_t/GDP_t$, an estimate of the dollar value impact of conservatism expressed as a percentage of gross domestic product.

$CONS_t = FAPrivate_{t-1} * \Theta_3 * News_t * (1 - Neg_t)$, where $FAPrivate_{t-1}$ is the current-cost value of net fixed assets owned by public and private businesses in year t–1 from Line 3 of Bureau of Economic Analysis (BEA) Fixed Asset Table 1.1; Θ_3 is an estimated regression coefficient from Model (2) presented in Table 3 that reflects the incremental sensitivity of aggregate corporate profits to bad news as compared to good news; and $News_t$ is the sum of monthly cash flow and discount rate shocks in year t formed from the first-order vector autoregression system presented in Table 1. Neg_t is a dummy variable that equals 1 if $News_t < 0$, and Neg_t equals 0 otherwise. See Section V for discussion.

GDP_t represents gross domestic product in year t from Line 1 of BEA National Income and Product Account (NIPA) Table 1.7.5.

Both $CONS_t$ and GDP_t have been converted into real year-2005 dollars using the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9. Larger positive $CONS_t/GDP_t$ values denote a greater percentage impact of accounting conservatism on GDP (e.g., a $CONS_t/GDP_t$ value of 1.0 percent indicates that real GDP in year t would have been approximately 1.0 percent higher if aggregate corporate profits incorporated good news in as timely a fashion as bad news).

The sample period begins in 1955 and ends in 2008. Shaded regions denote recessions as defined by the National Bureau of Economic Research.

In order to examine conservatism's influence on Fed decision making, I use a series of monetary policy reaction functions. A monetary policy reaction function is a macroeconomics tool that empirically links a policy instrument (e.g., the federal funds rate) with measurements of a central banker's objectives (e.g., inflation, GDP, unemployment) (Chappell, Havrilesky, and McGregor 1993). The reaction functions used in this study are based on the model of Fed behavior

formalized by Taylor (1993). Taylor (1993) empirically models the federal funds rate as a function of inflation and the output gap.[18]

An overview of my monetary policy tests is as follows. First, I estimate a baseline reaction function following the macroeconomics literature. Next, I modify the baseline reaction function to incorporate the dollar value impact of conservatism in order to determine whether the baseline or modified reaction function best describes actual Fed decision-making behavior.

Baseline Monetary Policy Reaction Function

As discussed above, I first estimate a baseline monetary policy reaction function following Taylor (1993):

$$\begin{aligned} FEDFUNDS_t = {} & \beta_0 + \beta_1 GAP1_t + \beta_2 INF_t + \beta_3 VOLCKER_t + \beta_4 GREENSPAN_t \\ & + \beta_5 BERNANKE_t + \varepsilon_t, \end{aligned} \tag{6}$$

where:

$FEDFUNDS_t$ = the nominal federal funds rate at the end of year t from Federal Reserve Statistical Release H.15;

$GAP1_t$ = a measure of the output gap in year t, defined as $GDPPotential_t - GDP_t$;[19]

$GDPPotential_t$ = an estimate of potential GDP (i.e., GDP if the economy was operating at full employment) in year t from the Congressional Budget Office;

GDP_t = gross domestic product in year t from Line 1 of BEA NIPA Table 1.7.5;[20]

INF_t = inflation, calculated as the percentage change in the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9 from year $t-1$ to year t; and

$VOLCKER_t$ ($GREENSPAN_t$) [$BERNANKE_t$] = dummy variables that equal 1 if Paul Volcker (Alan Greenspan) [Ben Bernanke] was the Chairman of the Federal Reserve in year t, and equal 0 otherwise.

All variables denominated in dollars have been converted into real (year-2005) dollars using the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9.

Table 4 presents descriptive statistics for the monetary policy reaction function variables. The sample period begins in 1955 (the first year that federal funds rate data are available) and ends in 2008. The federal funds rate averages 5.64 percent over the sample period. The mean *GAP1* value of 26.96 indicates that real economic output averages approximately $27 billion less than the level of output that could be obtained if the economy was operating at its highest sustainable level. The mean annual inflation rate (*INF*) is 3.6 percent over the sample period. Augmented Dickey-Fuller test statistics for *FEDFUNDS* and *GAP1* suggest that the federal funds rate and the output gap are stationary. However, the null hypothesis of a unit root for *INF* cannot be rejected in my sample. See Ang, Bekaert, and Wei (2007) for a summary of the debate in the macroeconomics literature as to whether inflation is stationary.

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[18] Reaction functions that model the federal funds rate using the output gap and inflation are commonly referred to as "Taylor rules." For a review of such rules, see Kozicki (1999), Taylor (1999), and Orphanides (2003).

[19] Positive (negative) GAP_t values indicate that the economy is operating below (above) its sustainable level. In other words, the economy has "room to grow" or is "overheated," respectively.

[20] Presumably, the Fed makes monetary policy decisions based on the current state of the economy and on expectations about the future. However, the Fed's real-time data and internal forecasts are generally proprietary. C. Romer and D. Romer (2000) and Sims (2002) show that the Fed's internal forecasts (i.e., the "Green Book") are high-quality predictors of *ex post* realizations. Thus, the literature generally relies on *ex post* realizations to proxy for Fed perceptions about the state of the economy when decisions are made (Orphanides 2004). Additionally, the mean revision to the first annual GDP estimate was 0.17 percent from 1983 to 2009 (Fixler, Greenaway-McGrevy, and Grimm 2011). This suggests that any look-ahead bias from the use of *ex post* realizations is minimal.

TABLE 5

Monetary Policy Reaction Functions

	Model (6)	Model (7)
n	54	54
R^2	0.749	0.763
Intercept	0.946	0.544
	(1.41)	(0.70)
GAP1	−0.009***	
	(−4.88)	
GAP2		−0.008***
		(−4.52)
INF	1.035***	1.111***
	(5.26)	(5.02)
VOLCKER	5.196***	5.521***
	(5.96)	(5.14)
GREENSPAN	1.289*	0.980
	(1.89)	(1.38)
BERNANKE	0.100	−0.031
	(0.25)	(−0.09)

Vuong (1989) Statistic: Model (6) vs. Model (7) = −2.21**

***, **, * Represent two-tailed significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
This table reports the results from a series of monetary policy reaction functions. The dependent variable in each specification is $FEDFUNDS_t$. The sample period begins in 1955 and ends in 2008. t-statistics are based on Newey and West's (1987) standard errors to control for potential autocorrelation in the residuals. The Vuong (1989) test statistic tests the null hypothesis that both models listed are equally close to the true model. A significantly positive (negative) test statistic rejects the null hypothesis in favor of the alternative hypothesis that the first (second) model listed is closer to the true model.
See Table 4 for variable definitions.

Table 5 presents parameter estimates for the baseline reaction function in Model (6). The negative and significant β_1 coefficient on *GAP* of –0.009 suggests that the Federal Reserve sets the federal funds rate 0.9 basis points lower for every $1 billion of output gap (i.e., the Fed maintains lower interest rates when the economy has "room to grow"). In contrast, the positive and significant β_2 coefficient on *INF* of 1.035 indicates that the Fed sets the federal funds rate 1.035 percent higher for every 1 percent of inflation. The positive and significant β_3 (β_4) coefficient on the *VOLCKER* (*GREENSPAN*) fixed effect variable of 5.196 (1.289) suggests that the federal funds rate was approximately 5.2 percent (1.3 percent) higher during the Paul Volcker (Alan Greenspan) chairmanship as compared to the pre-Volcker era, irrespective of the output gap and the inflation rate. The *BERNANKE* fixed effect is not significant. This lack of significance is not surprising given that the *BERNANKE* variable is non-zero for only three observations in the sample (2006–2008). The R^2 of 74.9 percent shows that the baseline reaction function can explain a significant portion of federal funds rate decisions. Overall, these results are consistent with prior research (Taylor 1993; Kozicki 1999; Ball and Tchaidze 2002).

Incorporating the Dollar Value Impact of Conservatism

In order to discern whether the Fed adjusts for the dollar value impact of conservatism on GDP when making monetary policy decisions, I modify the baseline reaction function above to incorporate the dollar value impact of conservatism. Specifically, I estimate the following:

$$FEDFUNDS_t = \Psi_0 + \Psi_1 GAP2_t + \Psi_2 INF_t + \Psi_3 VOLCKER_t + \Psi_4 GREENSPAN_t + \Psi_5 BERNANKE_t + \eta_t, \quad (7)$$

where $GAP2_t$ is defined as $GDPPotential_t - (GDP_t + CONS_t)$. In other words, $GAP2_t$ is an estimate of the output gap in year t if aggregate corporate profits incorporated good news in as timely a fashion as bad news. All variables denominated in dollars have been converted into real (year-2005) dollars using the implicit GDP deflator from Line 1 of BEA NIPA Table 1.1.9. Table 5 presents the results of Model (7). The coefficient on *INF* is slightly larger in absolute value compared to the corresponding coefficient within Model (6), while the coefficient on the adjusted output gap (*GAP2*) is slightly smaller in magnitude.

More importantly, the R^2 of 0.763 in Model (7) in Table 5 exceeds the R^2 in Model (6) of 0.749. While the 1.4 percent increase in explanatory power is modest, the implication that the Fed acts as if it adjusts for conservatism is economically meaningful. In order to test whether the difference in R^2 values between the two models is statistically significant, I perform a Vuong (1989) test. The Vuong (1989) statistic tests the null hypothesis that both models are equally close to the Fed's true (unobservable) decision rule. The Vuong (1989) statistic of −2.21 in Table 5 is significant at the 5 percent level. This suggests rejection of the null hypothesis in favor of the alternative hypothesis that Model (7) is closer to the true model than Model (6). In other words, the modified reaction function that incorporates the dollar value impact of conservatism on GDP better explains actual Fed decision behavior than a standard reaction function from the macroeconomics literature.

The results in Table 5 are consistent with the Fed adjusting for the impact of accounting conservatism on GDP when setting the federal funds rate. This leads to two natural follow-up questions. First, what is the source of the modified reaction function's superior explanatory power? Recall that $CONS = 0$ in bad news periods by construction (see Section V and the plot in Figure 2). As a result, $GAP1 = GAP2$ during bad news periods and, therefore, Models (6) and (7) generate identical results in bad news periods. Thus, the source of the modified reaction function's superior performance is solely due to the increased explanatory power in good news periods (i.e., when *CONS* is strictly positive, causing *GAP1* to differ from *GAP2*). Hence, the results in Table 5 are consistent with the Fed adjusting for the impact of conservatism in good news periods. Of course, it is also possible that the Fed may be adjusting for the impact of conservatism in bad news periods (i.e., aggregate corporate profits and GDP may be more sensitive to negative aggregate news than to positive aggregate news, but this does not imply that aggregate corporate profits and GDP fully reflect the impact of negative news). However, the construction of the *CONS* variable prevents me from detecting any Fed adjustment in bad news periods. Future research may be able to better detect potential Fed adjustment in bad news periods using more granular proxies for the dollar value impact of conservatism.

Second, how sophisticated is the Fed's response to the impact of accounting conservatism (i.e., is the Fed's response complete, or is the Fed over- or under-reacting to the impact of conservatism)? The structure of my empirical tests makes it difficult to make conclusive statements about whether the Fed's decision-making process is socially optimal. However, the descriptive statistics in Table 4 suggest that the Fed's response may be relatively complete. For example, the mean *GAP1* value of 26.96 is significantly positive. This suggests that, on average, the Fed sets the federal funds rate such that the economy operates at $27 billion below its sustainable level. However, the mean *GAP2* value is −3.44. Notably, this value is not significantly different from zero. This suggests that, after adjusting the output gap for the dollar value impact of conservatism, the Fed appears to set the federal funds rate such that the economy is operating at its sustainable level (i.e., the economy neither has room to grow, nor is it overheated). In other words, the Fed appears to be more successful in meeting its dual mission of promoting output while maintaining stable prices once the dollar value impact of conservatism on aggregate corporate profits and GDP is considered.

Robustness Checks

I perform a variety of (untabulated) robustness checks for the monetary policy reaction functions presented in Table 5. The aim is to determine whether the Taylor (1993) model best describes actual Fed behavior over my sample period (i.e., whether I am using the appropriate models from the macroeconomics literature). I modify the reaction functions in Models (6) and (7) in multiple ways, and the primary takeaways are as follows. First, adding the lagged federal funds rate, aggregate news (i.e., *News*), and the unemployment rate (whether individually or all at once) to either model reduces the explanatory power. Second, adding the growth in the Index of Leading Economic Indicators, the number of residential housing starts, and consumer confidence as additional explanatory variables (1) reduces the sample size due to data availability, and (2) introduces multicollinearity, which reduces the explanatory power of individual variables. Third, using lagged independent variables to ensure that the Fed has the specified realizations in their information set when making policy decisions also reduces explanatory power. Fourth, changes specifications of each model have lower explanatory power than the respective levels results. Thus, in summary, the monetary policy reaction functions based on Taylor (1993) best describe actual Fed decision making over my sample.

Summary

This section uses a series of monetary policy reaction functions based on Taylor (1993) in order to examine accounting conservatism's influence on Federal Reserve decision making. I show that incorporating the dollar value impact of conservatism increases the explanatory power of a monetary policy reaction function. These results are consistent with the Fed adjusting for the impact of accounting conservatism on GDP when setting the federal funds rate.

The results and inferences in this section are subject to caveats. First, researchers are generally constrained to observing central banker behavior and inferring the underlying decision rule (i.e., researchers are unable to truly get inside the Fed's "black box" of decision making). Moreover, I cannot consider every possible macroeconomic indicator that the Fed may use when setting interest rates. Thus, I do not claim that the Fed consciously or explicitly adjusts for the dollar value impact of accounting conservatism on macroeconomic indicators when making interest rate decisions. Additionally, I cannot definitively characterize any potential Fed adjustment as complete. I can only assert that if the Fed uses the variables that I model (and no others), then my results are at least consistent with the Fed acting as if GDP is a more timely reflection of macroeconomic conditions in bad news periods as compared to good news periods.

Second, my results do not speak to whether the Fed would prefer that certain macroeconomic indicators are asymmetrically sensitive to bad news as compared to good news. On one hand, conservatism may *improve* Fed decision making by providing the Fed with advance warning of deterioration in macroeconomic activity in much the same way that conservatism at the firm level provides bondholders with advance notice of deterioration in firm performance (see Watts 2003a). On the other hand, conservatism may *impair* Fed decision making by introducing an asymmetry into aggregate measurements that is costly to account for when executing monetary policy. For example, perhaps the Fed wants timely signals of underlying macroeconomic conditions (regardless of whether the news is good or bad) in order to take prompt action and foster growth while minimizing inflation.

VII. CONCLUSION

This study investigates the macroeconomic consequences of firm-level accounting conservatism. Consistent with conditional conservatism extending to the aggregate level, I demonstrate that

annual estimates of aggregate corporate profits and gross domestic product compiled by the U.S. Bureau of Economic Analysis are more sensitive to negative aggregate news than to positive aggregate news. Next, I estimate the dollar value impact of conservatism on measurements of macroeconomic fundamentals. Finally, I show that incorporating the dollar value impact of conservatism increases the explanatory power of a monetary policy reaction function that describes Federal Reserve interest rate decision behavior.

These results should be of interest to researchers and capital market participants. First, my results suggest that accounting has the potential to affect social welfare by influencing the measurement of key macroeconomic indicators and by shaping monetary policy decisions that regulate the money supply. Second, because changes in the firm-level definition of earnings (e.g., due to the increased use of fair value measurements or the proposed convergence between GAAP and IFRS) can alter the measurement of aggregate corporate profits and GDP, economic policy setters may be able to improve their decision making by better understanding how firm-level accounting measurements interact with the national economic accounts. Last, increased government regulation and intervention in global capital markets calls for research that examines how central planners, regulators, and other centralized economic decision makers use accounting information when making decisions outside of pure market settings.

Future research may investigate the social welfare implications of my results. For example, if the Fed fails to fully adjust for the impact of conservatism in good news periods, then GDP in good news periods may understate the true strength of the economy. Thus, the Fed may inadvertently set interest rates below the socially optimal level in an effort to spur growth, which could lead to future inflation. Hence, one avenue for further research may be modeling past dollar value impacts of conservatism as determinants of future output and inflation.

One example of current research that investigates related questions is Li and Shroff (2010), who investigate whether financial reporting quality leads to faster macroeconomic growth in an international setting. Additionally, Nallareddy and Ogneva (2014) find that aggregated accounting information can be used to predict revisions to macroeconomic indicators, thereby potentially improving economic decision making. Similarly, Bushman, Piotroski, and Smith (2011) investigate the relation between capital allocation decisions and the timely recognition of economic losses across countries. See, also, Leuz and Wysocki (2008) for suggestions regarding future research on the macro effects of firms' reporting and disclosure behavior.

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Are CEOs and CFOs Rewarded for Disclosure Quality?

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ABSTRACT: This study provides evidence regarding the importance that boards of directors place on effective communication with the investor community by examining whether CEO and CFO compensation are related to the quality of the firm's financial disclosures. Using an index derived from analyst forecast characteristics and management forecast accuracy to measure disclosure quality, we find changes in the annual bonus for both the CEO and CFO to be positively associated with changes in disclosure quality. We also find that the relation is stronger for high-growth firms, firms that have stronger governance structures, and for executives with lower equity incentives. Overall, our findings provide insight into the importance that boards place on effective communication with investors as a responsibility of the CEO and CFO and, therefore, provide them with contractual incentives to address the moral hazard problem associated with voluntary disclosures.

Keywords: *executive compensation; voluntary disclosure; corporate governance.*

JEL Classifications: *M41.*

I. INTRODUCTION

Prior research documents that a firm's chief executive officer (CEO) and chief financial officer (CFO) are responsible for the firm's interactions with the investment community.[1] However, investor relations is only one of the CEO's and CFO's responsibilities, and it could be argued that other actions, such as developing strategic, operating, and investment policies,

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[1] Examples include Francis, Nanda, and Olsson (2008), Bowen, Davis, and Matsumoto (2002, 2005), Rogers and Stocken (2005), Anderson, Duru, and Reeb (2009), and Bamber, Jiang, and Wang (2010).

have more direct implications for the firm's cash flows and firm value. Given those responsibilities, the issue of whether boards of directors consider the provision of high-quality financial disclosures to be a sufficiently important responsibility to incorporate within the firm's incentive structure is an open question. To provide empirical evidence on that question, we examine whether there is a positive relation between the quality of a firm's financial disclosures and bonus payments to the CEO and CFO, and the conditions that affect the strength of that association.

Excerpts from proxy statements, reported in Appendix A, provide anecdotal evidence that boards hold the CEO responsible for the firm's relations with various stakeholders, including investors. For convenience and consistency with prior literature, throughout this paper, we use the term "disclosure quality" to refer to the overall effectiveness of communications with the investment community, as opposed to the disclosure of specific information. This distinction is important in our setting because if the board considers the disclosure of an item, in and of itself, to be value-enhancing, then the board could direct management to make the disclosure. However, the quality of the information provided is subjective and dependent on stochastic outcomes. To provide managers with an incentive to obtain and disclose high-quality information, the board needs to link the executive's pay to his or her assessment of disclosure quality. For example, while the board could direct management to issue a forecast, it cannot direct management to issue an accurate forecast. Therefore, boards are more likely to reward managers based on the accuracy of forecasts rather than the number of forecasts issued.

Agency theory suggests that boards should link compensation to disclosure quality if they consider communications with investors to be an important value-increasing responsibility for the executive officers (Holmstrom 1979).[2] According to agency models, an efficient performance measure should be associated with output (increase in firm value), under the control of the agent and costly to produce. There is evidence to suggest that disclosure quality satisfies these conditions. The link between disclosure quality and firm value comes from its impact on cost of capital (Botosan 1997), litigation risk (Francis, Philbrick, and Schipper 1994), and investment efficiency (Bushman and Smith 2001; Biddle and Hilary 2006). The CEO's and CFO's control over communications with the investor community is supported by their participation in conference calls with analysts and approval of the text of earnings announcements, including forecasts of earnings. Finally, the personal costs of providing high-quality information include both the effort required to obtain high-quality information and the reduction in the executives' ability to extract rents (Anderson et al. 2009).

However, from a practical perspective, boards choose from a finite set of performance measures, and the CEOs and CFOs have a broad range of responsibilities. Even if improving disclosure quality is a value-increasing activity, placing an incentive weight on disclosure quality can be counterproductive if the marginal product of the manager's effort is higher in other areas. In addition, the absence of a well-defined, generally accepted, objective measure of disclosure quality could preclude boards from rewarding managers for improving disclosure quality.[3] As a result, the

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[2] Prior studies have examined the impact of the quality of a firm's financial disclosures on CEO turnover with mixed results. Lee, Matsunaga, and Park (2012) document evidence that when overall firm financial performance is poor, boards use management forecast error as a confirmatory signal regarding the CEO's ability. In contrast, Agrawal, Jaffe, and Karpoff (1999) and Hennes, Leone, and Miller (2008) do not find evidence that executive turnover is associated with accounting errors or fraud.

[3] As discussed below, the lack of a well-defined measure for disclosure quality suggests that boards will implicitly reward managers for improving disclosure quality, as opposed to explicitly including a measure of disclosure quality in the executive's bonus contract. In agency theory, the concept of noise refers to the extent to which a measure reflects the agent's actions. If the board does not believe that they can effectively distinguish changes in the firm's disclosure quality, then they will not use disclosure quality to determine the executive's pay (Baker, Gibbons and Murphy 1994).

issue of whether boards use the firm's disclosure quality as an implicit nonfinancial performance measure in determining CEO/CFO pay is an empirical question.

A second question relates to conditions under which we expect firms to provide stronger incentives for effectively communicating with investors and analysts. In this regard, we test two predictions. First, the relation should be stronger for high-growth firms because the greater degree of information asymmetry and investor interest increases the demand for high-quality financial information (Khurana, Pereira, and Martin 2006; Bushman, Indjejikian, and Smith 1996). Second, the relation should be stronger for firms with more effective corporate governance structures because stronger boards are more likely to design optimal compensation structures that incorporate nonfinancial performance measures (Ittner, Larcker, and Rajan 1997).

To test these predictions, we use a sample of firms drawn from the ExecuComp database for 1996–2010 and relate CEO and CFO bonus payments to two measures of financial disclosure quality. Our first measure is an index that combines three analysts' forecast characteristics that Lang and Lundholm (1996) show are related to analysts' assessments of the quality of a firm's financial disclosures. Our second measure is management forecast accuracy (Healy and Palepu 2001; Ajinkya, Bhojraj, and Sengupta 2005). To isolate the effect of environmental factors, we regress each measure against a set of variables that capture the firm's overall information environment and use the residual as our measure of disclosure quality. Thus, the measures used in our compensation tests reflect the quality of the firm's financial disclosures that are not explained by environmental factors.[4]

Consistent with our hypotheses, we find significant relations between changes in each of our disclosure quality measures and changes in bonus for both the CEO and CFO. We also find the relation to be concentrated in firms with high sales growth and firms with strong corporate governance. These results are consistent with boards of directors considering investor communications to be an important value-enhancing responsibility for CEOs and CFOs and with the importance of investor communications being higher for high-growth firms and firms with stronger governance structures.

We conduct two additional tests to support the validity of our primary results. First, we do not find a corresponding significant relation for the Chief Operating Officer (COO), who does not have investor relations responsibilities and, second, we find that the management forecast accuracy results are concentrated in cases in which reported earnings exceed the management forecast. The latter result provides evidence that forecast accuracy does not proxy for overall financial performance because when earnings exceed the forecast, stronger financial performance reduces forecast accuracy.

While our findings are consistent with boards reducing moral hazard by providing an incentive to exert effort to generate high disclosure quality, they are also consistent with the managerial labor market using disclosure quality to update beliefs regarding managerial quality. To distinguish between the two competing explanations, we conduct a series of three tests. First, following Banker, Darrough, Huang, and Plehn-Dujowich (2013), we examine whether there is a positive relation between changes in disclosure quality and the change in future salary. Second, assuming that the uncertainty regarding an individual's ability is inversely related to the individual's age, we examine whether the relation is stronger for younger executives. Third, given that equity-based incentives reduce the extent of the underlying moral hazard problem of managerial disclosure, we examine whether the relation between disclosure quality and bonus payments is stronger when equity incentives are lower. We do not find a significant relation between changes in disclosure quality and changes in future salaries, or evidence that the relation between disclosure quality and annual bonus

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[4] Results are robust if we adopt a single-stage approach.

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is stronger for younger executives than for older executives. In contrast, we find the relation between disclosure quality and the annual bonus to be stronger when the equity incentives are lower. Taken together, our results favor the agency theory hypothesis over the reservation wage hypothesis.

Our study contributes to the literature by providing evidence that boards consider effective communications with investors to be a sufficiently important responsibility for CEOs and CFOs to incorporate it into the executive's compensation structure. While prior evidence suggests that disclosure quality can increase firm value, it is not clear that boards recognize its importance. The positive relation between disclosure quality and managers' pay suggests that boards consider the quality of a firm's financial disclosures to be an important driver of firm value.

We also contribute to the literature on the role of corporate governance and managerial incentives on disclosure quality (Bushee and Noe 2000; Nagar, Nanda, and Wysocki 2003; Ajinkya et al. 2005; Karamanou and Vafeas 2005; Anderson et al. 2009; Lee et al. 2012). As discussed by Beyer, Cohen, Lys, and Walther (2010), there is a considerable literature investigating whether disclosure and corporate governance are substitutes or complements. Our study suggests that firms with stronger governance structures are more likely to tie bonuses to disclosure quality in order to encourage effective communications (Armstrong, Guay, and Weber 2010).

Third, our study adds evidence regarding the implications of disclosure quality on firm value (Botosan 1997; Healy and Palepu 2001; Francis et al. 2008) by documenting that the relation between bonus payments and disclosure quality is stronger for growth firms. Our evidence is consistent with investors considering disclosure quality to be more important when the underlying uncertainty and information asymmetry are higher (Khurana et al. 2006).

The remainder of our paper is organized as follows. Section II reviews the literature and develops our hypotheses. Section III outlines our methodology and the measurement of our test variables. Section IV reports our results, and Section V concludes.

II. HYPOTHESIS DEVELOPMENT

One of the key responsibilities of boards is to provide efficient incentives to management to reduce the underlying agency problem (Jensen and Meckling 1976). While equity-based grants provide broad incentives for managers to take actions that increase firm value (Core and Guay 1999), agency theory (Holmstrom 1982; Banker and Datar 1989; Bushman and Indjejikian 1993; Sloan 1993; etc.) states that efficient contracts reduce risk-sharing costs by using bonus plans tied to specific measures of performance. A useful performance measure provides an efficient signal regarding the manager's contribution to firm output and should be linked to a manager's pay if the board believes that it is related to firm value and is under the manager's control, i.e., reflects the manager's actions with a relatively small amount of noise.

Traditional agency theory also assumes an efficient labor market under which managers are paid their reservation wage, where the reservation wage represents the individual's marginal product (contribution to firm value) at their next best alternative. To set the market wage, the labor market uses public signals regarding the manager's ability in order to assess the manager's marginal product for their particular firm. If competitor firms consider an outcome as a reliable signal regarding managerial ability, then the reservation wage will vary along with the performance measure.

Both mechanisms lead to similar predictions in that they rely on the perception that the manager has control over the quality of a firm's financial disclosures, and that communicating financial information to the investor community is an important determinant of firm value. If those assumptions hold and high-quality financial information is costly to produce and disseminate, then the board should tie managers' compensation to the quality of the firm's financial disclosures. Thus, both internal and external forces could provide managers with incentives to increase disclosure quality.

The Relation between Disclosure Quality and Firm Value

Prior studies indicate that high-quality disclosures increase firm value by reducing cost of capital (Botosan 1997; Francis et al. 2008; Easley and O'Hara 2004), lowering litigation risk (Francis et al. 1994), and improving investment efficiency (Bushman and Smith 2001; Biddle and Hilary 2006). Diamond and Verrecchia (1991) model the link between disclosure quality and firm value and show that stronger disclosure quality can reduce a firm's cost of capital by increasing the liquidity of the firm's shares. Botosan (1997) provides empirical evidence that stronger disclosure, as measured by the information disclosed in annual reports, reduces firms' cost of capital. Francis, LaFond, Olsson, and Schipper (2004) document evidence of a negative relation between accrual quality and cost of capital. Easley and O'Hara (2004) show that information risk is priced and is associated with higher expected returns.

In addition to lowering the cost of capital, disclosing higher-quality information can increase cash flows by increasing the efficiency of investment (Bushman and Smith 2001; Biddle and Hilary 2006). This occurs because the additional transparency makes it easier for outsiders to identify poor investment decisions. The monitoring reduces agency costs and encourages managers to make better investments.[5]

This argument is also consistent with signaling models. The provision of high-quality information requires managers to have a strong understanding of the underlying economic and competitive environment faced by the firm and to foresee how the firm will be able to succeed in that environment. Because similar skills are useful in making effective strategic and operating decisions, the quality of a firm's financial disclosures signals the manager's ability to increase firm value (Chang, Dasgupta, and Hilary 2010). Therefore, the quality of the firm's financial disclosures shifts the reservation wage in the labor market, leading to a positive relation between disclosure quality and pay.

Costs of Providing High Disclosure Quality

The underlying theory also assumes that the production of high-quality disclosures imposes costs on the individual managers. One implication of this assumption is that, without incentives, managers would underinvest in activities that would increase disclosure quality and, therefore, produce disclosure quality below the optimal level.

The costs of providing high-quality disclosures include the effort required to obtain, analyze, and communicate information. High-quality disclosures also effectively bond managers by increasing transparency, and managers forgo the opportunity to bias disclosures in a self-serving manner. For example, high-quality disclosures reduce the ability of executives to increase the value of their options by disclosing biased information (Aboody and Kasznik 2000; Bartov and Mohanram 2004) or otherwise profit by indulging in insider trading (Cheng and Lo 2006). Therefore, better disclosure quality mitigates managers' opportunistic reporting to extract rents.

Constraints on Using Disclosure Quality as a Performance Measure

Despite the theoretical support for linking executive pay to the quality of information disclosed, practical considerations could inhibit boards from incorporating disclosure quality in the firm's incentive structure. Boards must select a relatively small number of measures that best capture the agent's contribution to firm value. An overly complex contract is likely to create

[5] Hermalin and Weisbach (2012) make a similar argument in a different context. Specifically, they model disclosure as exogenous and argue that stronger disclosure requirements reduce managers' ability to extract wealth from the firm and that managers would, therefore, require additional pay to compensate them for their effort.

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confusion and make it difficult for the agent to identify the optimal action. Thus, boards should only include the quality of disclosed financial information as a performance measure if its impact on firm value is sufficiently high and the quality of disclosure is under the manager's control.

In addition, communicating with the investment community is only one of the executives' responsibilities and boards may not want to divert the managers' attention from other activities that have a stronger or more direct impact on firm value. CEO time and effort may be more productively spent in formulating product strategies rather than addressing questions from analysts or increasing the accuracy of earnings forecasts. The board should only place a positive weight on the quality of investor communications in setting executive pay if it considers relations with investors to be a sufficiently important responsibility for the officer. Finally, there is no clear, objective standard for measuring the relevance, accuracy, and precision of financial information communicated to investors. Therefore, it is difficult to explicitly contract on disclosure quality. Instead, firms have to rely on implicit contracts that reward managers for perceived increases in the quality of the firm's financial communications (Hayes and Schaefer 2000).

Overall, we investigate whether boards consider disclosure quality to be an important, value-increasing responsibility of the CEO and CFO by investigating whether there is a positive association between their bonuses and the quality of the firm's financial disclosures. The formal hypothesis, stated in alternative form, is:

H1: *Ceteris paribus,* CEO and CFO bonuses are positively associated with the quality of the firm's financial disclosures.

The strength of the association is also likely to differ based on firm characteristics. Firms in the growth stage of their development are more likely to use the quality of a firm's financial disclosures as a performance measure in response to a greater demand for financial information from capital providers. Growth firms tend to generate a greater amount of investor interest and information asymmetry. Consistent with this argument, Miller (2002) documents evidence of a positive association between earnings increases and discretionary disclosure, and Khurana et al. (2006) find that growth firms have stronger disclosure policies. In addition, because disclosure quality reflects the executives' ability to understand the underlying competitive environment and effectively anticipate future outcomes, higher disclosure quality could signal the executive's ability to enhance firm value. Thus, we would expect growth firms to provide stronger incentives to CEOs to improve the firm's financial disclosure quality.

We also expect stronger, higher-quality boards to be more likely to recognize the importance of financial disclosure and to invest the time and effort to incorporate idiosyncratic nonfinancial performance measures into executive incentive plans. This is consistent with arguments and empirical evidence reported in Ajinkya et al. (2005) and Karamanou and Vafeas (2005) that firms with higher-quality governance are more likely to provide higher-quality financial disclosures. It is also consistent with the evidence in Ittner et al. (1997) that the use of nonfinancial performance measures is negatively related to CEOs' power over the board.[6]

This leads to our second hypothesis (stated in alternative form):

H2a: *Ceteris paribus,* the relation between CEO and CFO bonuses and the quality of the firm's financial disclosures is stronger for growth firms.

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[6] As noted by Brickley and Zimmerman (2010), finding a single measure of "good governance" is problematic. In addition, the benefits of disclosure could be lower when monitoring is high, i.e., disclosure and governance could be substitutes. Each of these effects would reduce the power of the tests. Because our hypothesis assumes an efficient incentive contract, we expect the hypothesis is more likely to hold when the board is stronger (more independent, etc.).

H2b: *Ceteris paribus,* the relation between CEO and CFO bonuses and the quality of the firm's financial disclosures is stronger for firms with stronger, higher-quality boards.

III. SAMPLE AND MEASUREMENT OF MAIN VARIABLES

Sample Selection

Panel A of Table 1 describes the sample selection procedure. The sample is drawn from the ExecuComp database for the period 1996–2010. Because the dependent variable is the change in bonus, we require the same individual executive to be present for two consecutive years. In addition, we eliminate firm-years for which required Compustat data are not available, and firms with negative stockholders' equity. Finally, we require firms to have analyst forecasts available on I/B/E/S and at least two analysts following the firm during the year. This leads to a sample of 1,970 firms and 12,247 firm-year observations for tests of CEO compensation that use the index measure based on analyst forecasts, and 1,127 firms and 5,169 firm-year observations for tests that use management forecast accuracy. Because the availability of compensation data is lower, the sample is smaller for tests on the CFO and COO.

Panels B and C of Table 1 present the distribution of firm-year observations by year and by industry, respectively. Although there is a general increase in the number of observations over time, observations are fairly evenly spread over the entire sample period. In addition, the industry distribution appears to be comparable to the industry composition of firms in the intersection of the Compustat and I/B/E/S databases.

General Methodology

Consistent with prior research, our regressions control for ΔROA and stock market return and condition on frequency of meeting forecasts (Lambert and Larcker 1987; Sloan 1993; Baber, Janakiraman, and Kang 1996; Matsunaga and Park 2001; Hartzell and Starks 2003). This procedure recognizes that boards of directors generally retain the discretion to adjust pay based on unspecified or implicit measures of performance, such as meeting earnings benchmarks (Matsunaga and Park 2001) or subjective and discretionary individual performance evaluations (Bushman et al. 1996). This discretion allows the firm to adjust executive pay for performance-related objectives that do not have a commonly accepted definition, such as financial disclosure quality.

We use the change in the manager's annual bonus as our dependent variable. Although the bonus represents one component of the individual's total compensation, unlike equity incentives, the bonus is the aspect of the compensation package that is likely to be most sensitive to current performance and to nonfinancial performance measures.[7] The change specification reduces the potential bias from omitted variables that influence the magnitude of compensation.

Our first measure of the disclosure quality uses analyst forecast characteristics to derive an overall, indirect measure. Lang and Lundholm (1996) show that accuracy of analyst forecasts, the dispersion of analyst forecasts, and the variance of analyst forecast revisions are related to the analysts' ratings regarding a firm's financial disclosures. They examine ratings from the Report of the Financial Analysts Federation Corporate Information Committee (1985–1989), in which analysts evaluate firms based on annual published information, other published information, and investor relations. They find that the rankings are associated with more accurate analyst forecasts, less dispersion among analysts, and less volatility in forecast revisions, and conclude that analyst forecast characteristics reflect overall

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[7] We also conduct the tests using the change in cash compensation rather than change in bonus. The qualitative results are similar.

TABLE 1

Sample Selection and Description

Panel A: Sample Selection Procedure

	Number of Firm-Years	Number of Firms
ExecuComp CEO compensation data and CEO with tenure over two years from 1996 to 2010	22,405	2,989
I/B/E/S analyst forecast data and at least two analysts following during the year	16,466	2,432
Compustat and CRSP data for general disclosure quality test	12,247	1,970
Compustat and CRSP data for management forecast test	5,169	1,127

Panel B: Number of Observations by Year

Year	Disclosure Index Sample	Management Forecast Sample
1996	688	70
1997	686	87
1998	696	114
1999	702	138
2000	710	164
2001	746	348
2002	812	411
2003	830	435
2004	879	461
2005	869	474
2006	870	481
2007	889	489
2008	935	509
2009	957	507
2010	978	481
Total	12,247	5,169

Panel C: Industry Composition

Industry Classification	Sample Percentage	Compustat/I/B/E/S Percentage
Agriculture and Forestry (01–09)	0.14	0.26
Mining (10–14)	4.29	3.51
Construction (15–17)	1.49	1.41
Manufacturing (20–39)	45.51	42.55
Telecommunication (48)	1.29	1.94
Wholesale (50–51)	2.92	3.12
Retail (52–59)	8.70	8.25
Services (70–88)	12.55	14.86
Other	23.25	24.10
Total	100.00	100.00

disclosure quality as viewed by analysts. As the survey rankings are not available, we use the analyst forecast characteristics to construct a measure of the overall quality of the firm's financial disclosures. Following Anderson et al. (2009), we aggregate the individual measures by constructing a disclosure index for each year that is the sum of the decile rankings of the three measures, divided by 30.[8] The index is scaled such that a higher value represents higher disclosure quality.

Our second measure is management forecast accuracy. Management forecast accuracy is considered as an important indication of disclosure quality by boards (Ajinkya et al. 2005; Karamanou and Vafeas 2005). Beyer et al. (2010) find that approximately 66 percent of accounting-based information is provided by management forecasts. Following prior studies (e.g., Hui, Matsunaga, and Morse 2009), we compute the difference between the first management forecast of annual earnings and actual reported earnings and scale the absolute value of the difference by share price as of the beginning of the year. To convert the variable into one that is increasing in accuracy, we multiply the absolute scaled management forecast error by -1.[9]

Panel A of Table 2 presents descriptive data for testing variables. Variable definitions are listed in Appendix B. The mean (median) percentage change in *Bonus* is 2.5 percent (1.1 percent) for CEOs, which is slightly greater than the mean (median) of 1.9 percent (0.5 percent), for CFOs. The *Disclosure Index* has a median of 0.533 and an interquartile range of 0.2, while management forecast accuracy has a median of 0.004 and an interquartile range of 0.011. Panels B and C of Table 2 present the pairwise correlations for the variables used in the empirical tests for the CEO and CFO samples, respectively. The two measures of the quality of the firm's financial disclosures are positively correlated, as expected. The correlation is stronger for the smaller CFO sample, with the Pearson correlation rising from 0.103 in the CEO sample to 0.196 in the CFO sample. We also find significantly positive correlations between each disclosure measure and the change in CFO bonus. However, in the larger CEO sample, only the Pearson correlation between the change in the disclosure index and change in CEO bonus is significant.

First-Stage Regression

We use a first-stage regression to remove the effects of public information from other sources and private information acquisition on analysts' forecasts and the effects of underlying firm risk, or predictability of earnings on management forecast accuracy. The residual from the regression should represent management's influence on each measure:

$$\begin{aligned} \textit{Disclosure Quality} = {} & \alpha_0 + \alpha_1 \textit{LnAsset}_{it} + \alpha_2 \textit{Market-to-Book}_{it} + \alpha_3 \textit{ROA}_{it} + \alpha_4 \textit{Institutions}_{it} \\ & + \alpha_5 \textit{Leverage}_{it} + \alpha_6 \textit{Loss}_{it} + \alpha_7 \textit{ROA Volatility}_{it} + \alpha_8 \textit{Analyst Coverage}_{it} \\ & + \alpha_9 \textit{Foreign Sales}_{it} + \alpha_{10} \textit{Insider Ownership}_{it} + \alpha_{11} \textit{R\&D}_{it} \\ & + \alpha_{12} \textit{Segments}_{it} + \alpha_{13} \textit{Shareholders}_{it} + \alpha_{14} \textit{ROA}_{it+1} + \textit{Industry Dummies} \\ & + \textit{Year Dummies} + e_1, \end{aligned} \tag{1}$$

where:

Disclosure Quality = either the disclosure index or management forecast accuracy;
LnAssets = log of total assets;

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[8] Anderson et al. (2009) construct their opacity index using trading volume, bid-ask spread, analyst following, and analyst forecast error. In an unreported sensitivity test, we use the opacity index as a measure of disclosure quality. The results are qualitatively similar.

[9] Management forecast accuracy measures the disclosure quality by using *ex post* realized earnings. Alternatively, we use forecast frequency as a measure of disclosure. We find that (untabulated) changes in forecast frequency are positively associated with bonus changes for both CEOs and CFOs.

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TABLE 2
Measure of Financial Disclosure Effectiveness

Panel A: Variable Distribution Statistics

	Mean	Std. Dev.	25%	50%	75%
Compensation Measures					
CEOΔBonus	0.025	0.191	−0.175	0.011	0.071
CFOΔBonus	0.019	0.157	−0.035	0.005	0.047
Disclosure Measures					
Disclosure Index	0.547	0.136	0.433	0.533	0.633
Analyst Forecast Error	0.009	0.037	0.003	0.001	0.004
Forecast Dispersion	0.055	0.138	0.008	0.017	0.040
Revision Volatility	0.006	0.018	0.001	0.002	0.004
ΔDisclosure	−0.001	0.137	−0.088	−0.001	0.089
Management Forecast Accuracy	0.011	0.018	0.001	0.004	0.012
ΔAccuracy	−0.040	0.785	−0.352	−0.001	0.360
Control Variables First-Stage					
LnAssets	7.530	1.671	6.302	7.361	8.621
Market-to-Book	3.320	2.975	1.664	2.425	3.851
ROA	0.037	0.101	0.013	0.044	0.083
Institutions (%)	65.645	20.737	51.514	66.880	80.689
Leverage	0.176	0.157	0.029	0.151	0.283
Loss	0.251	0.434	0.000	0.000	1.000
ROA Volatility	0.067	0.112	0.013	0.031	0.071
Analyst Coverage	2.007	0.932	1.609	2.197	2.708
Foreign Sales	0.020	0.073	0.000	0.000	0.000
Insider Ownership (%)	0.053	0.115	0.005	0.014	0.043
R&D	0.031	0.062	0.000	0.000	0.031
Segments	0.697	0.688	0.000	0.693	1.386
Shareholders	24.734	68.424	1.155	4.725	17.000
Control Variables Second-Stage					
ΔROA	−0.009	0.072	−0.018	0.000	0.013
Return	0.073	0.449	−0.211	0.042	0.287
ΔChange Misses	0.047	1.288	−1.000	0.000	1.000
ΔChange Declines	0.037	1.672	−1.000	0.000	1.000
ΔChange Losses	0.001	0.709	0.000	0.000	0.000
ΔSales Growth	−0.018	0.103	−0.056	−0.011	0.029
Conditional Variables					
Sales Growth	0.141	0.159	0.043	0.107	0.201
Governance	0.421	0.202	0.300	0.421	0.556

Panel B: Correlations among Testing Variables (CEO Sample).

	ΔBonus–CEO	*ΔDisclosure*	*ΔAccuracy*	*ΔROA*	*Return*	*ΔMisses*	*ΔDeclines*	*ΔLosses*	*ΔSales Growth*
ΔBonus–CEO		0.015	0.004	**0.253**	**0.255**	**−0.217**	**−0.276**	**−0.139**	**0.113**
ΔDisclosure	**0.055**		**0.101**	−0.034	**0.060**	**−0.053**	**−0.116**	−0.031	−0.009
ΔAccuracy	0.021	**0.103**		−0.027	0.039	**−0.162**	**−0.175**	**−0.150**	**0.052**
ΔROA	**0.160**	−0.005	−0.038		**0.344**	**−0.193**	**−0.184**	**−0.138**	**0.192**
Return	**0.225**	**0.046**	0.033	**0.319**		**−0.108**	**−0.079**	−0.046	**0.124**

(continued on next page)

TABLE 2 (continued)

	ΔBonus–CEO	*ΔDisclosure*	*ΔAccuracy*	*ΔROA*	*Return*	*ΔMisses*	*ΔDeclines*	*ΔLosses*	*ΔSales Growth*
ΔMisses	**−0.223**	**−0.054**	**−0.169**	**−0.199**	**−0.117**		**0.401**	**0.198**	**−0.065**
ΔDeclines	**−0.251**	**−0.115**	**−0.160**	**−0.174**	**−0.078**	**0.412**		**0.302**	**−0.124**
ΔLosses	**−0.130**	−0.025	**−0.171**	**−0.143**	**−0.053**	**0.211**	**0.301**		**−0.110**
ΔSales Growth	**0.081**	0.017	**0.079**	**0.143**	**0.087**	**−0.066**	**−0.114**	**−0.129**	

Panel C: Correlations among Testing Variables (CFO Sample)

	ΔBonus–CFO	*ΔDisclosure*	*ΔAccuracy*	*ΔROA*	*Return*	*ΔMisses*	*ΔDeclines*	*ΔLosses*	*ΔSales Growth*
ΔBonus–CFO		**0.058**	**0.057**	**0.201**	**0.194**	**−0.188**	**−0.238**	**−0.095**	**0.081**
ΔDisclosure	**0.053**		**0.121**	−0.027	**0.062**	**−0.047**	**−0.109**	−0.040	−0.017
ΔAccuracy	**0.065**	**0.196**		−0.030	0.017	**−0.137**	**−0.199**	**−0.160**	0.046
ΔROA	**0.122**	−0.043	−0.018		**0.368**	**−0.229**	**−0.179**	**−0.173**	**0.217**
Return	**0.164**	0.043	0.039	**0.326**		**−0.121**	**−0.117**	−0.026	**0.189**
ΔMisses	**−0.184**	**−0.049**	**−0.150**	**−0.181**	**−0.169**		**0.403**	**0.202**	**−0.070**
ΔDeclines	**−0.212**	**−0.108**	**−0.161**	**−0.260**	**−0.152**	**0.406**		**0.285**	**−0.131**
ΔLosses	**−0.088**	−0.018	**−0.139**	**−0.177**	**−0.124**	**0.208**	**0.280**		**−0.104**
ΔSales Growth	**0.075**	0.016	**0.073**	**0.187**	**0.142**	**−0.075**	**−0.122**	**−0.121**	

This table provides descriptive information regarding the distributions and correlations for the variables used in our empirical analysis. In Panels B and C, the numbers below the diagonal are the Pearson correlations, and numbers above the diagonal are Spearman correlations. Bold figures are statistically significant from 0 at the 5 percent (two-tailed) level.

Market-to-Book = market-to-book ratio of equity;
ROA = earnings before extraordinary items divided by total assets;
Institutions = percentage of institutional holdings;
Leverage = ratio of book value of debt to book value of assets;
Loss = 1 if the firm recorded a loss, and 0 otherwise;
ROA Volatility = the standard deviation of change in annual return on assets for the six-year period immediately prior to the current year;
Analyst Coverage = number of analysts following the firm;
Foreign Sales = foreign sales divided by total assets;
Insider Ownership = the percentage of outstanding shares owned by officers and directors;[10]
R&D = research and development expenses divided by total assets;
Segments = log of the number of business segments; and
Shareholders = number of common shareholders (in thousands).

We expect larger firms, more successful firms, firms with higher growth prospects, and firms that draw the attention of institutional investors to generate public information from press coverage. Because conservatism may cause earnings to reflect performance with a delay (Collins, Kothari, Shanken, and Sloan 1994), we include both contemporaneous and future ROA. The leverage ratio is a general control for the information acquisition activities and monitoring of debt holders and the impact of financial leverage on firm risk. The loss dummy variable captures the lower relevance of accounting earnings associated with loss firms (Hayn 1995). The ROA volatility variable reflects

[10] We obtain the number of shares held by officers and directors from the Thomson Reuters "Insider Filing Data Feed," which aggregates and summarizes data from Forms 3, 4, 5, and 144 filings.

the uncertainty regarding future income. *Foreign Sales* and *R&D* intensity are controls for the complexity of the firm's underlying operations. The number of segments captures the complexity of operating in multiple product markets, which would make earnings more difficult to forecast, or the diversification effect of multiple product markets, which would make earnings easier to forecast. *Insider Ownership* and the number of shareholders control for the firm's ownership structure. Income statement variables are for the current year and all other variables are measured as of the beginning of the fiscal year. The regression also includes fixed effects for the year and for the industry (based upon the two-digit SIC industry groups).

Table 3 presents results from the estimation of Equation (1). The overall explanatory power of the model is relatively strong for both disclosure measures, with adjusted R^2s of 0.270 and 0.228 for the *Disclosure Index* and *Management Forecast Accuracy,* respectively. The coefficients on the log of total assets, market-to-book ratio, percentage of institutional holdings, and number of analysts are generally significantly positive, consistent with those variables representing the extent of public information available about the firm. The significantly negative coefficients on the loss indicator and ROA volatility are consistent with expectations, and the significantly negative coefficient on financial leverage is consistent with leverage increasing firm risk. The coefficient on contemporaneous *ROA* is significantly positive for both measures and the coefficient on future *ROA* is significantly positive for the *Disclosure Index.* These results are consistent with successful firms drawing more press coverage and being easier to forecast. The significantly negative coefficients for *Foreign Sales* and *R&D* intensity in the *Disclosure Index* regression are consistent with international operations and research activities leading to greater disagreement among analysts. The significantly positive coefficient on *Insider Ownership* in the disclosure quality regression is consistent with nonfinancial incentives leading insiders to release high-quality information. Finally, the significantly positive coefficient on the number of segments suggests that the diversification effect of operating in different product markets makes it easier to forecast earnings.

IV. RESEARCH DESIGN AND RESULTS

Test of H1

To test our hypotheses, we examine the impact of information quality on CEO compensation using the following regression:

$$\begin{aligned}\Delta Bonus_{it} = {} & \beta_0 + \beta_1 \Delta Disclosure_{it} + \beta_2 (HiQuality_{it} \times \Delta ROA_{it}) + \beta_3 \Delta ROA_{it} + \beta_4 Return_{it} \\ & + \beta_5 (HiQuality_{it} \times Return_{it}) + \beta_6 \Delta Misses_{it} + \beta_7 \Delta Declines_{it} + \beta_8 \Delta Losses_{it} \\ & + \beta_9 (HiQuality_{it} \times \Delta Misses_{it}) + \beta_{10} (HiQuality_{it} \times \Delta Declines_{it}) \\ & + \beta_{11} (HiQuality_{it} \times \Delta Losses_{it}) + \beta_{12} \Delta SalesGrowth_{it} + Industry\ Dummies \\ & + Year\ Dummies + e_2,\end{aligned} \tag{2}$$

where:

$\Delta Bonus$ = the annual bonus in year t scaled by total compensation for year t less the annual bonus in year t–1 scaled by total compensation in year t–1;
$\Delta Disclosure$ = residual from the regression in Equation (1) for year t less the residual for year t–1;
$HiQuality$ = 1 if the residual from the regression of Equation (1) for year t is above the sample median, and 0 otherwise;
ΔROA = return on assets for year t less return on assets for year t–1;
$Return$ = annualized stock return for year t;

TABLE 3

Regression of Disclosure Quality on Information Environment Variables
Estimated Coefficient (t-statistic)

	Expected Sign	*Disclosure Index* (1)	*Management Forecast Accuracy* (2)
LnAssets	+	0.005***	0.036
		(2.91)	(1.38)
Market-to-Book	+	0.004***	−0.008
		(7.03)	(−1.07)
Institutions	+	0.000***	0.000
		(2.77)	(0.30)
Leverage	+/−	−0.035***	−0.094
		(−3.34)	(−0.46)
Loss	−	−0.057***	−0.237***
		(−23.21)	(−4.56)
ROA Volatility	−	−0.086***	−0.553**
		(−5.79)	(−2.22)
Analyst Coverage	+	0.007***	0.171***
		(2.30)	(3.11)
ROA	+/−	0.202***	6.545***
		(11.13)	(6.50)
Foreign Sales	−	−0.040**	−0.732
		(−2.13)	(−1.41)
Insider Ownership	+	0.040***	−0.121
		(3.21)	(−0.33)
R&D	−	−0.121***	−0.441
		(−3.38)	(−0.66)
Segments	−	0.006***	0.076**
		(2.70)	(2.21)
Shareholders	+	0.000	−0.000
		(0.91)	(−0.44)
Future ROA	+	0.182***	0.122
		(11.68)	(0.24)
Constant	?	−0.616***	−0.536
		(−21.26)	(−1.60)
Adjusted R^2		0.270	0.228
Number of Observations		14,211	6,317

, * Indicate statistical significance at the 5 percent and 1 percent levels, respectively, two-tailed.
The dependent variable in Column (1) (*Disclosure Index*) is derived from analyst forecast characteristics. The dependent variable in Column (2) is the *Management Forecast Accuracy*. The regression results include industry and year fixed effects (not reported). Robust standard errors are clustered at the firm level.
Variable definitions are described in Appendix B.

Δ*Misses* = number of quarters the firm missed the consensus analyst forecast during year *t* less the number of quarters the firm missed the consensus analyst forecast in year *t*−1;

Δ*Declines* = number of quarters the firm reported EPS below the same quarter of the preceding year less the number of quarters the firm reported lower EPS in year *t*−1;

$\Delta Losses$ = number of quarters in which the firm reported earnings below zero in year t less the number of quarters in which the firm reported losses in year t–1; and

$\Delta SalesGrowth$ = the change of sales growth of year t over that of year t–1, where sales growth of year t is measured over three years from t–1 to t+1.

Industry dummies using two-digit SIC and year dummies are included.

In this model, the variable *Disclosure* is the residual from either the disclosure index or management forecast accuracy regression (Equation (1)). H1 predicts that the change in disclosure quality is positively related to the change in bonus, i.e., $\beta_1 > 0$.

Models such as Banker and Datar (1989) and Bushman and Indjejikian (1993) show that the relative weight on earnings is a function of the relative noise in the performance measures. To allow the weight of *ROA, Return,* and meeting benchmarks to vary with the firm's disclosure quality, we define an indicator variable, *HiQuality,* that is set equal to 1 if the value of *Disclosure* is above the sample median and interact it with the performance measures.[11] To control for incentives to expand the firm's market share, we include the change in sales growth as an additional performance measure.

Table 4 presents the results for the test of H1's prediction that CEO and CFO bonuses are positively associated with disclosure quality. Our results support the hypothesis. For each executive officer, we find significantly positive coefficients for the change in the disclosure index (at the 1 percent level) and for the change in management forecast accuracy (at the 5 percent level). We also find that the coefficients for the two executive officers are similar, which is consistent with both officers being held responsible for the quality of the firm's financial disclosures. To assess the economic impact, a 25 percent change in the disclosure index corresponds to a 20 percent change in the bonus, i.e., the change in bonus for the CEO (approximately $24,000) and a 22 percent change in the dependent variable for the CFO (approximately $8,800).[12]

For both disclosure quality measures in Table 4, we find that coefficients on the interactions of *HiQuality* with *ROA* and *Return* are insignificant. The lack of evidence that the weights on *ROA* or *Return* are conditional on disclosure quality seems inconsistent with the findings of De Franco, Hope, and Larocque (2013), who find that firms place a higher weight on both accounting and stock returns when the firm provides more guidance. However, while De Franco et al. (2013) focus on the extent of disclosure, our tests focus on the quality of the firm's financial disclosures.[13] Consistent with Matsunaga and Park (2001), we find that CEO and CFO bonus payments are a function of the number of quarters during the year that the firm reports earnings below the quarterly consensus analyst forecast or below the reported earnings in the same quarter of the preceding year.

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[11] The use of a dummy variable facilitates interpretation of coefficients of the interaction terms and mitigates multicollinearity in the regression. Results are qualitatively similar if we use the continuous measure of disclosure quality, or use the rank measure disclosure quality. Our findings are robust if we exclude the interaction terms of *HiQuality* in Equation (1).

[12] These amounts reflect sample-wide averages. In some of the subsamples below, the coefficients on disclosure quality and, hence, the economic impact for the officer, are substantially higher. The economic magnitudes are comparable to those associated with other performance measures. For example, for the CEO (CFO), the changes in cash associated with a 25 percent increase in ΔROA, *Return,* and $\Delta SalesGrowth$ are $11,000 ($3,000), $65,000 ($16,000), and $23,000 ($8,500), respectively.

[13] De Franco et al. (2013) compare firms that issue management forecasts to firms that do not. In our management forecast tests, we restrict our analysis to the subset of firms that issue forecasts and consider the accuracy of the forecasts issued.

TABLE 4

The Relation between Effective Disclosure Policies and CEO and CFO Bonuses
Estimated Coefficient (t-statistic)

	Disclosure Index		*Management Forecast Accuracy*	
	CEO (1)	**CFO (2)**	**CEO (3)**	**CFO (4)**
ΔDisclosure	0.038***	0.040***	0.077**	0.074**
	(2.71)	(2.76)	(2.22)	(2.10)
HiQuality × ΔROA	−0.016	−0.017	−0.197	−0.278
	(−0.29)	(−0.29)	(−1.36)	(−1.65)
ΔROA	0.099**	0.079*	0.302**	0.308**
	(2.40)	(1.95)	(2.12)	(2.18)
Return	0.061***	0.041***	0.065***	0.058***
	(9.10)	(6.11)	(4.98)	(4.30)
HiQuality × Return	0.006	−0.003	−0.007	−0.019
	(0.68)	(−0.41)	(−0.41)	(−1.03)
ΔMisses	−0.015***	−0.011***	−0.013***	−0.006
	(−6.39)	(−4.64)	(−2.88)	(−1.34)
ΔDeclines	−0.018***	−0.015***	−0.019***	−0.014***
	(−12.21)	(−10.11)	(−6.93)	(−4.60)
ΔLosses	−0.008**	−0.001	0.012	−0.005
	(−2.27)	(−0.15)	(1.30)	(−0.58)
HiQuality × ΔMisses	−0.003	−0.002	−0.004	−0.005
	(−1.10)	(−0.68)	(−0.68)	(−0.91)
HiQuality × ΔDeclines	0.001	0.005**	0.003	0.002
	(0.48)	(2.28)	(0.78)	(0.43)
HiQuality × ΔLosses	0.000	−0.003	−0.001	0.026*
	(0.04)	(0.45)	(−0.07)	(1.92)
ΔSalesGrowth	0.054***	0.060***	0.045	0.079**
	(3.06)	(3.26)	(1.60)	(2.12)
Adjusted R^2	0.193	0.172	0.200	0.145
Number of Observations	12,247	8,939	5,169	3,839

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents the results of OLS regressions. The dependent variables are the bonus in year t scaled by total compensation in year t less the bonus in year $t-1$ scaled by total compensation for year $t-1$. In the disclosure index tests, the *ΔDisclosure* independent variable is the change in disclosure quality not explained by environmental factors and is calculated as the residual from the Table 3. In the management forecast accuracy tests, the *ΔDisclosure* independent variable is the residual from the Table 3 regression using management forecast accuracy for year t minus that for year $t-1$. Industry and year fixed effects are included and not reported. Robust standard errors are clustered at the firm level. All other variable definitions are described in Appendix B.

Tests of H2a and H2b

H2a predicts that the relation will be stronger for growth firms. To identify high-growth firms, we use the three-year average sales growth from years $t-1$ to $t+1$. This provides a contemporaneous measure of growth experienced by the firm during the period in which the firm is making its

disclosure choices and the board is setting compensation.[14] H2a predicts that financial disclosures are more important for firms experiencing higher growth and boards of such firms assign a higher weight to disclosure quality in determining compensation.

To allow the coefficient on every performance measure to vary across sales growth, we split the sample at the median sales growth and separately estimate the regressions for the high and low sales growth samples. We report the results for the disclosure index measure of disclosure quality (Panel A) and management forecast accuracy measure (Panel B) in Table 5. We report the results separately for the CEO (Columns (1) and (2)) and the CFO (Columns (3) and (4)).

For the disclosure index (Panel A of Table 5), our results are consistent with H2a in that we find significantly positive coefficients for the change in disclosure for the high sales growth samples (t-statistics = 3.46 and 3.05 for the CEO and CFO, respectively). In contrast, for the low growth sample, the coefficients on the change in disclosure index are insignificant (t-statistics = 0.17 and 0.14 for the CEO and CFO, respectively). For both executives, F-tests indicate that the changes in disclosure quality coefficients are significantly larger for the high-growth sample.

We find similar results when we use management forecast accuracy as the measure of disclosure quality (Panel B of Table 5). The coefficients on the change in forecast accuracy are significantly positive for both officers in the high sales growth sample (t-statistics = 2.45 and 2.39 for the CEO and CFO, respectively) and insignificant in the low sales growth sample (t-statistics = –0.08 and –0.17, for the CEO and CFO, respectively). In addition, for both officers, the difference in coefficients across the two samples is significant. Thus, our results are consistent with the board assigning a larger weight to financial disclosure quality in determining CEO and CFO bonus payments when the firm is experiencing higher growth, suggesting that boards consider financial transparency more important when growth is higher.

H2b predicts that the relation between disclosure quality and compensation is greater for firms with strong governance systems. To measure governance quality, we obtain data from the Corporate Library and follow Karamanou and Vafeas (2005) by using factor analysis to construct a governance index based on board independence, board size, and number of board meetings. We split the sample at the median value and separately estimate the regressions for the strong and weak governance samples.

For the disclosure index (Panel A of Table 6), our results are consistent with H2b in that we find significantly positive coefficients of change in disclosure for the high governance quality samples (t-statistics = 2.59 and 3.23 for the CEO and CFO, respectively) and insignificant coefficients for the low governance quality samples (t-statistics = –0.20 and 0.17 for the CEO and CFO, respectively). The coefficient for the change in disclosure quality is significantly larger for the high governance quality sample than for the low governance quality sample for each executive.

We find similar results for management forecast accuracy (Panel B of Table 6). Coefficients for the change in forecast accuracy are significantly positive in the high governance quality sample (t-statistics = 2.25 and 2.17 for the CEO and CFO, respectively) and insignificant in the low sales growth sample (t-statistics = 0.11 and –0.45 for the CEO and CFO, respectively). For both officers, the difference in coefficients across the two samples is significant. Thus, our results are consistent with higher-quality boards placing a greater weight on financial disclosure quality in determining CEO and CFO bonuses.

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[14] We choose sales growth over R&D expenditures and the market-to-book ratio because R&D expenditures are only relevant for certain industries and the market values are a function of disclosure quality.

TABLE 5
Partitions Based on Sales Growth
Estimated Coefficient (t-statistic)

Panel A: Disclosure Index

Independent Variable	CEO: High Sales Growth (1)	CEO: Low Sales Growth (2)	CFO: High Sales Growth (3)	CFO: Low Sales Growth (4)
ΔDisclosure	0.073***	0.003	0.051***	0.002
	(3.46)	(0.17)	(3.05)	(0.14)
HiQuality × *ΔROA*	−0.024	0.007	−0.049	0.031
	(−0.28)	(0.09)	(−0.55)	(0.40)
ΔROA	0.117**	0.070	0.081	0.066
	(2.07)	(1.11)	(1.48)	(1.09)
Return	0.053***	0.080***	0.037***	0.046***
	(5.66)	(6.95)	(4.64)	(4.39)
HiQuality × *Return*	0.003	0.011	0.003	−0.016
	(0.26)	(0.72)	(0.32)	(−1.16)
ΔMisses	−0.015***	−0.015***	−0.011***	−0.010***
	(−4.33)	(−4.55)	(−3.50)	(−3.10)
ΔDeclines	−0.019***	−0.019***	−0.013***	−0.017***
	(−8.52)	(−8.09)	(−6.14)	(−7.90)
ΔLosses	−0.010*	−0.007	−0.008*	0.005
	(−1.68)	(−1.53)	(−1.73)	(0.99)
HiQuality × *ΔMisses*	0.002	−0.010**	−0.001	−0.004
	(0.54)	(−2.05)	(−0.25)	(−0.81)
HiQuality × *ΔDeclines*	0.002	0.002	0.002	0.008**
	(0.51)	(0.46)	(0.72)	(2.56)
HiQuality × *ΔLosses*	0.002	−0.001	0.000	−0.003
	(0.23)	(−0.11)	(0.06)	(−0.39)
ΔSalesGrowth	0.067***	0.030	0.067***	0.025
	(3.03)	(0.83)	(3.10)	(0.70)
Adjusted R^2	0.173	0.193	0.173	0.175
Number of Observations	6,125	6,122	4,469	4,470
F-test				
ΔDisclosure	(1) vs. (2)		(3) vs. (4)	
F-stat	4.20**		4.88**	
(p-value)	(0.04)		(0.03)	

Panel B: Management Forecast Accuracy

Independent Variable	CEO: High Sales Growth (1)	CEO: Low Sales Growth (2)	CFO: High Sales Growth (3)	CFO: Low Sales Growth (4)
ΔAccuracy	0.104**	−0.004	0.091**	−0.003
	(2.45)	(−0.08)	(2.39)	(−0.17)
HiAccuracy × *ΔROA*	−0.371	−0.068	−0.730***	−0.051
	(−1.39)	(−0.42)	(−3.12)	(−0.22)

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TABLE 5 (continued)

Independent Variable	CEO High Sales Growth (1)	CEO Low Sales Growth (2)	CFO High Sales Growth (3)	CFO Low Sales Growth (4)
ΔROA	0.392	0.235	0.589***	0.161
	(1.45)	(1.63)	(2.74)	(0.87)
Return	0.065***	0.056***	0.069***	0.056***
	(3.41)	(3.29)	(3.42)	(3.12)
HiAccuracy × *Return*	−0.003	−0.004	−0.022	−0.020
	(−0.14)	(−0.19)	(−0.76)	(−0.85)
ΔMisses	−0.006	−0.020***	−0.010*	−0.003
	(−0.99)	(3.55)	(−1.71)	(−0.45)
ΔDeclines	−0.032***	−0.011***	−0.017***	−0.012***
	(−6.57)	(−3.52)	(−3.28)	(−3.03)
ΔLosses	0.001	0.015	0.013	−0.011
	(0.03)	(1.48)	(0.68)	(−1.11)
HiAccuracy × *ΔMisses*	−0.014*	0.009	−0.007	−0.002
	(−1.85)	(1.22)	(−0.96)	(−0.22)
HiAccuracy × *ΔDeclines*	0.008	0.000	0.002	0.001
	(1.34)	(0.02)	(0.22)	(0.23)
HiAccuracy × *ΔLosses*	0.013	−0.010	0.001	0.033**
	(0.59)	(−0.67)	(0.04)	(2.13)
ΔSalesGrowth	0.011	0.070*	0.073*	0.079
	(0.27)	(1.63)	(1.32)	(1.57)
Adjusted R^2	0.217	0.186	0.179	0.104
Number of Observations	2,584	2,584	1,920	1,919
F-test				
ΔAccuracy	(1) vs. (2)		(3) vs. (4)	
F-stat	6.92***		7.01***	
(p-value)	(0.00)		(0.00)	

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents the results of OLS regressions. The dependent variables are the bonus in year *t* scaled by total compensation in year *t* less the bonus in year *t*−1 scaled by total compensation for year *t*−1. High/low sales growth is the subsample with average three-year sales growth from year *t*−1 to year *t*+1 above/below the sample median. Industry and year fixed effects are included, but not reported. Robust standard errors are clustered at the firm level.
All other variables are described in Appendix B.

COO Compensation

Although the change specification partially controls for correlated omitted variables, to further address concerns regarding unidentified firm-specific effects, we focus on a sample of firms that have compensation data available for the Chief Operating Officer (COO), as well as the Chief Executive and Chief Financial Officer. Because the COO is less responsible for the firm's financial disclosure policies, we would expect to see a weaker relation between bonus and disclosure quality.

Restricting the sample to firms for which data are available for all three officers reduces the sample size to 3,837 firm-year observations and to 1,724 firm-year observations for the tests using the disclosure index and management forecast accuracy, respectively. In Panel A of Table 7, we find that coefficients on change in the disclosure index remain significantly positive in the reduced

TABLE 6

Partitions Based on Governance
Estimated Coefficient (t-statistic)

Panel A: Disclosure Index

Independent Variable	CEO High Governance (1)	CEO Low Governance (2)	CFO High Governance (3)	CFO Low Governance (4)
ΔDisclosure	0.051***	−0.003	0.070***	0.012
	(2.59)	(−0.20)	(3.23)	(0.17)
HiQuality × ΔROA	−0.092	−0.003	−0.138	0.025
	(−1.14)	(−0.04)	(−1.43)	(0.32)
ΔROA	0.154**	0.071	0.148**	0.041
	(2.38)	(1.23)	(2.00)	(0.79)
Return	0.072***	0.054***	0.039***	0.040***
	(7.64)	(5.50)	(4.38)	(4.11)
HiQuality × Return	−0.003	0.014	0.002	−0.002
	(−0.28)	(1.12)	(0.13)	(−0.13)
ΔMisses	−0.014***	−0.014***	−0.006*	−0.013***
	(−4.42)	(−4.05)	(−1.85)	(−3.63)
ΔDeclines	−0.021***	−0.016***	−0.017***	−0.014***
	(−9.94)	(−7.02)	(−8.15)	(−5.82)
ΔLosses	−0.005	−0.012**	−0.005	0.001
	(−1.06)	(−2.48)	(−1.07)	(0.21)
HiQuality × ΔMisses	−0.004	−0.005	−0.004	−0.003
	(−0.93)	(−0.99)	(−1.02)	(−0.52)
HiQuality × ΔDeclines	0.003	0.001	0.007**	0.002
	(0.90)	(0.16)	(2.40)	(0.73)
HiQuality × ΔLosses	0.000	0.001	−0.002	−0.001
	(0.00)	(0.17)	(−0.25)	(−0.16)
ΔSalesGrowth	0.039	0.066***	0.045	0.079***
	(1.46)	(2.68)	(1.61)	(3.03)
Adjusted R^2	0.194	0.199	0.191	0.167
Number of Observations	5,879	5,879	4,252	4,255
F-test				
ΔDisclosure	(1) vs. (2)		(3) vs. (4)	
F-stat	8.07***		5.48**	
(p-value)	(0.00)		(0.02)	

Panel B: Management Forecast Accuracy

Independent Variable	CEO High Governance (1)	CEO Low Governance (2)	CFO High Governance (3)	CFO Low Governance (4)
ΔAccuracy	0.127**	0.004	0.121**	−0.019
	(2.25)	(0.11)	(2.17)	(−0.45)
HiAccuracy × ΔROA	0.198	−0.401*	−0.336	−0.258
	(0.81)	(−1.92)	(−1.42)	(−1.23)

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TABLE 6 (continued)

Independent Variable	CEO High Governance (1)	CEO Low Governance (2)	CFO High Governance (3)	CFO Low Governance (4)
ΔROA	−0.384*	0.597***	0.378**	0.210
	(−1.66)	(3.18)	(2.11)	(1.13)
Return	0.063***	0.028	0.088***	0.042***
	(3.18)	(1.13)	(3.27)	(2.67)
HiAccuracy × *Return*	−0.014	0.019	−0.064*	0.004
	(−0.49)	(0.57)	(−1.95)	(0.19)
ΔMisses	−0.008	−0.022**	−0.010	−0.005
	(−1.12)	(−2.35)	(−1.38)	(−0.92)
ΔDeclines	−0.030***	−0.038***	−0.002	−0.020***
	(−4.57)	(−4.74)	(−0.48)	(−4.96)
ΔLosses	−0.017	−0.001	−0.008	−0.005
	(−0.91)	(−0.11)	(−0.81)	(−0.45)
HiAccuracy × *ΔMisses*	−0.006	0.000	−0.011	0.000
	(−0.58)	(0.02)	(−1.18)	(0.03)
HiAccuracy × *ΔDeclines*	0.014*	0.021**	0.002	−0.002
	(1.70)	(2.26)	(0.34)	(−0.27)
HiAccuracy × *ΔLosses*	0.007	0.024	0.046**	0.012
	(0.30)	(1.02)	(2.50)	(0.65)
ΔSalesGrowth	0.091*	−0.099	0.109	0.070
	(1.76)	(−1.48)	(1.31)	(1.51)
Adjusted R^2	0.170	0.246	0.170	0.138
Number of Observations	1,919	1,919	1,520	1,521
F-test				
ΔAccuracy	(1) vs. (2)		(3) vs. (4)	
F-stat	4.537**		6.60**	
(p-value)	(0.03)		(0.01)	

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table presents the results of OLS regressions. The dependent variables are the bonus in year *t* scaled by total compensation in year *t* less the bonus in year *t*−1 scaled by total compensation for year *t*−1. The high-governance sample includes observations in which the governance score is above the sample median, and the low-governance sample includes observations in which the governance score is below the sample median. The governance score is derived from a factor analysis of board independence, board size, and number of board meetings. Industry and year fixed effects are included, but not reported. Robust standard errors are clustered at the firm level.
All other variables are defined in Appendix B.

sample, with t-statistics = 2.24 and 3.54 for the CEO and CFO, respectively, and the coefficient for the COO is not statistically significant (t-statistic = −0.72). The coefficient on the change in disclosure index is also significantly lower for the COO than for the CEO and CFO. In contrast, the coefficients on variables that reflect overall financial performance (*ΔROA* and *Return*) do not significantly differ for the COO. We find similar results in Panel B of Table 7 with management forecast accuracy. Coefficients on the change in forecast accuracy are significantly positive for the CEO and CFO (t-statistics = 2.02 and 2.48, respectively), while the coefficient for the COO is not significant (t-statistic = −0.11). The coefficient on the change in forecast accuracy for the COO is

TABLE 7

Comparisons of Executive Officers' Change in Bonus Estimated Coefficient (t-statistic)

Panel A: Disclosure Index

Independent Variable	CEO (1)	CFO (2)	COO (3)
ΔDisclosure	0.026**	0.042***	−0.013
	(2.24)	(3.54)	(−0.72)
HiQuality × ΔROA	−0.024	−0.044	−0.109
	(−0.29)	(−0.59)	(−0.96)
ΔROA	0.054	0.083*	0.072
	(0.95)	(1.68)	(0.96)
Return	0.048***	0.031***	0.045***
	(5.00)	(4.17)	(3.69)
HiQuality × Return	0.013	0.006	0.001
	(1.02)	(0.69)	(0.11)
ΔMisses	−0.011***	−0.007**	−0.009**
	(−2.93)	(−2.39)	(−2.64)
ΔDeclines	−0.021***	−0.017***	−0.019***
	(−8.96)	(−8.78)	(−7.42)
ΔLosses	−0.004	−0.007	−0.004*
	(−0.71)	(−1.53)	(−0.79)
HiQuality × ΔMisses	−0.007	−0.009**	−0.008
	(−1.36)	(−2.32)	(−1.69)
HiQuality × ΔDeclines	0.002	0.005	0.003
	(0.63)	(1.54)	(0.88)
HiQuality × ΔLosses	−0.016*	−0.005	−0.006
	(−1.77)	(−0.64)	(−0.77)
ΔSalesGrowth	−0.008	0.005	−0.019
	(−0.36)	(0.26)	(−0.73)
Adjusted R^2	0.198	0.199	0.163
Number of Observations	3,837	3,837	3,837
F-test		(1) vs. (3)	(2) vs. (3)
ΔDisclosure	F-stat	6.63***	16.77***
	(p-value)	(0.01)	(0.00)
ΔROA	F-stat	0.06	2.38
	(p-value)	(0.81)	(0.12)
Return	F-stat	0.11	0.04
	(p-value)	(0.74)	(0.83)

Panel B: Management Forecast Accuracy

Independent Variable	CEO (1)	CFO (2)	COO (3)
ΔAccuracy	0.032**	0.059**	−0.017
	(2.02)	(2.48)	(−0.11)
HiAccuracy × ΔROA	0.208	0.133	0.020
	(1.18)	(0.66)	(0.14)

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TABLE 7 (continued)

Independent Variable	CEO (1)	CFO (2)	COO (3)
ΔROA	0.203*	0.138	0.150
	(1.71)	(1.26)	(1.52)
Return	0.079***	0.066***	0.066***
	(4.45)	(3.98)	(3.69)
HiAccuracy × Return	0.026	0.019	0.049*
	(1.00)	(0.89)	(1.86)
ΔMisses	−0.021***	−0.012***	−0.014***
	(−3.88)	(−2.67)	(−2.78)
ΔDeclines	−0.021***	−0.016***	−0.016***
	(−5.15)	(−4.79)	(−4.33)
ΔLosses	0.019	0.008	0.008
	(1.51)	(0.86)	(0.88)
HiAccuracy × ΔMisses	0.006	0.002	0.003
	(0.66)	(0.24)	(0.30)
HiAccuracy × ΔDeclines	0.000	0.001	−0.005
	(0.00)	(0.25)	(−1.03)
HiAccuracy × ΔLosses	−0.005	0.007	−0.002
	(−0.24)	(0.41)	(−0.08)
ΔSalesGrowth	−0.062	−0.009	0.025
	(−1.21)	(−0.18)	(0.38)
Adjusted R^2	0.227	0.213	0.209
Number of Observations	1,724	1,724	1,724
F-test		(1) vs. (3)	(2) vs. (3)
ΔAccuracy	F-stat	5.61**	6.54**
	(p-value)	(0.02)	(0.01)
ΔROA	F-stat	0.98	0.00
	(p-value)	(0.32)	(0.95)
Return	F-stat	0.80	0.03
	(p-value)	(0.37)	(0.86)

Panel C: Growth and Governance Partitions—Disclosure Score

	Sales Growth		*Governance*	
Independent Variable	**High (1)**	**Low (2)**	**High (3)**	**Low (4)**
ΔDisclosure	−0.040	0.001	−0.011	−0.028
	(−1.50)	(0.04)	(−0.30)	(−1.07)
HiQuality × ΔROA	−0.053	−0.115	−0.329	−0.083
	(−0.37)	(−0.90)	(−1.06)	(−0.89)
ΔROA	−0.017	0.129	0.187	0.058
	(−0.23)	(1.14)	(0.90)	(0.74)
Return	0.056***	0.036**	0.051**	0.056***
	(3.80)	(2.19)	(2.21)	(3.74)
HiQuality × Return	−0.011	0.007	0.013	−0.019
	(−0.60)	(0.42)	(0.54)	(−1.10)
ΔMisses	−0.010**	−0.007	−0.010	−0.008
	(−1.97)	(−1.34)	(−1.58)	(−1.31)

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TABLE 7 (continued)

Independent Variable	*Sales Growth* High (1)	*Sales Growth* Low (2)	*Governance* High (3)	*Governance* Low (4)
ΔDeclines	−0.021***	−0.016***	−0.020***	−0.013***
	(−6.28)	(−4.16)	(−4.68)	(−3.40)
ΔLosses	−0.002	−0.011	−0.008	−0.003
	(−0.36)	(−1.16)	(−0.83)	(−0.48)
HiQuality × *ΔMisses*	−0.002	−0.015*	−0.004	−0.011
	(−0.36)	(−1.87)	(−0.59)	(−1.22)
HiQuality × *ΔDeclines*	0.001	0.006	0.004	−0.002
	(0.12)	(1.01)	(0.81)	(−0.33)
HiQuality × *ΔLosses*	−0.011	0.004	−0.021	−0.004
	(−1.07)	(0.32)	(−1.32)	(−0.43)
ΔSalesGrowth	−0.048	0.002	−0.067	0.029
	(−1.20)	(0.08)	(−1.37)	(0.74)
Adjusted R^2	0.194	0.141	0.174	0.164
Number of Observations	1,919	1,918	1,550	1,549
F-test		(1) vs. (2)	(3) vs. (4)	
ΔDisclosure	F-stat	1.58	0.13	
	(p-value)	(0.21)	(0.72)	

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table provides results from OLS regressions. The dependent variables are the bonus in year *t* scaled by total compensation in year *t* less the bonus in year *t*−1 scaled by total compensation for year *t*−1. We estimate separate regressions for the Chief Executive Officer (CEO), Chief Financial Officer (CFO), and Chief Operating Officer (COO). The regressions are based on a common sample for which data for all three officers are available. Industry and year fixed effects are included, but not reported. Robust standard errors are clustered at the firm level.
All variables are defined in Appendix B.

significantly different from coefficients for the CEO and CFO, whereas the coefficients for the other performance measures are not significantly different for the individual officers.

Finally, we examine whether the coefficients on the change in disclosure vary across sales and growth partitions for the COO in a manner similar to the findings reported in Tables 5 and 6 for the CEO and CFO. The results (Panel C of Table 7) show that, in contrast to the results for the CEO and CFO, the coefficients on the change in disclosure for the COO are not significant in any of the partitions and that the coefficients are not significantly different across the partitions.

Overall, the results are consistent with the COO being held responsible, along with the CEO and CFO, for overall firm performance, but not for the quality of the firm's financial disclosures. These findings suggest that boards incorporate financial disclosure quality into the incentive compensation scheme for officers who are most responsible for financial disclosures, thereby providing additional confidence that the prior results are not driven by firm-specific omitted variables.

Conditioning Management Forecast on Missing the Forecast

In the prior tests, we evaluate the impact of forecast accuracy regardless of whether reported earnings are greater or less than the management forecast. However, because forecast error is determined by firm performance, as well as the quality of the forecast, the board could view positive and negative forecast errors differently. Therefore, we interact management forecast accuracy with

an indicator variable set equal to 1 if reported earnings are below the forecast. This test also provides an additional control for overall financial performance. We report the results in Table 8.

The coefficients in Table 8 for the interaction of management forecast accuracy and the missed forecast indicator are significantly negative (t-statistics = −1.71 and −2.28 for the CEO and CFO, respectively). This indicates that management forecast accuracy has a larger impact on the annual bonus when actual earnings exceed the forecast. This reduces concerns that prior results are not driven by overall financial performance, because when actual earnings exceed the forecast, stronger financial performance reduces the accuracy of the forecast. One implication of this result is that the CEO and CFO bonuses are lower if the actual earnings exceed the forecast by a large margin (i.e., when the forecast is less accurate), thereby penalizing managers for providing a low-ball forecast. In addition, F-tests indicate that when the reported earnings fall below the forecast, management forecast accuracy does not have a significant effect on the bonus payments for the CEO or CFO. Taken together, these results suggest that managers do not want to issue a forecast that is too low (they are penalized if they exceed the forecast by too much) or too high (they are penalized if they miss the forecast), thus supporting the view that incentive plans provide an incentive to provide accurate forecasts.

Incentives versus Signal of Ability

To distinguish between the moral hazard and reservation wage adjustment economic explanations, we conduct three tests. First, we assume that salary adjustments reflect changes in the reservation wage (Banker et al. 2013) and examine whether there is a significant relation between changes in disclosure quality and future changes in salary. A positive relation would serve as evidence in favor of the reservation wage adjustment explanation. Second, we examine whether the strength of the relation between disclosure quality and bonus differs based on the executive's age. If the change in bonus reflects changes in the reservation wage, and the change in disclosure quality serves as a signal of the manager's ability, then we expect the results to be stronger when there is more uncertainty regarding the individual's ability. Assuming that there is greater uncertainty for younger executives, a finding that the relation is stronger for younger executives would favor the reservation wage adjustment explanation. Finally, we examine whether the strength of the relation between changes in disclosure quality and changes in bonus differs depending on the executive's equity incentives. Assuming that the moral hazard problem is greater when equity incentives are lower, evidence of a stronger relation when equity incentives are lower would favor the agency theory explanation.

To examine the relation between changes in disclosure quality and future salary adjustments, we estimate the main regression with the percentage change in salary from t to $t+1$ as the dependent variable. We present the results for both measures of disclosure and both executive officers in Panel A of Table 9. The coefficient on the change in disclosure quality is not significant in any of the regressions.

To test whether the relation between disclosure quality and bonus is stronger for younger executives, we split the sample at the sample median age of the executive and estimate the regressions separately for the younger and older samples. We present the results for the disclosure quality index in Panel B of Table 9 and the results for management forecast accuracy in Panel C of Table 9. We do not find evidence that the relation between disclosure quality and bonus is stronger for younger executives. The differences in the coefficients on disclosure quality are generally insignificant across the two samples. The lone significant difference (Panel B of Table 9) indicates that the relation is stronger for the older CFOs than the younger CFOs.[15]

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[15] This result could be due to the time horizon agency problem being stronger for older CFOs. We also conducted the test using tenure (defined as the first year in which the executive appears in ExecuComp) with similar results.

TABLE 8

Conditioning on Missing Management Forecasts
Changes in Bonus
Estimated Coefficient (t-statistic)

Independent Variable	CEO (1)	CFO (2)
ΔAccuracy	0.120***	0.146***
	(2.65)	(3.26)
ΔAccuracy × MFMiss	−0.083*	−0.131**
	(−1.71)	(−2.28)
MFMiss	−0.012*	−0.000
	(−1.77)	(−0.07)
HiAccuracy × ΔROA	−0.224	−0.334*
	(−1.54)	(−1.94)
ΔROA	0.304**	0.343**
	(2.13)	(2.42)
Return	0.063***	0.058***
	(4.76)	(4.28)
HiAccuracy × Return	−0.008	−0.020
	(−0.48)	(−1.12)
ΔMisses	−0.012***	−0.007
	(−2.74)	(−1.43)
ΔDeclines	−0.019***	−0.014***
	(−6.84)	(−4.59)
ΔLosses	0.012	−0.005
	(1.30)	(−0.54)
HiAccuracy × ΔMisses	−0.003	−0.005
	(−0.57)	(−0.77)
HiAccuracy × ΔDeclines	0.003	0.002
	(0.91)	(0.55)
HiAccuracy × ΔLosses	−0.002	0.025*
	(−0.15)	(1.84)
ΔSalesGrowth	0.043	0.078**
	(1.52)	(2.09)
Adjusted R^2	0.201	0.146
Number of Observations	5,169	3,839
F-test		
ΔAccuracy + (*ΔAccuracy × MFMiss*)		
F-stat	0.72	0.10
(p-value)	(0.40)	(0.75)

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table provides results from OLS regressions. The dependent variables are the bonus in year *t* scaled by total compensation in year *t* less the bonus in year *t*−1 scaled by total compensation for year *t*−1. *MFMiss* is an indicator variable equal to 1 if the reported earnings (I/B/E/S) were below the management forecast, and 0 otherwise. Industry and year fixed effects are included, but not reported. Robust standard errors are clustered at the firm level.
All other variables are defined in Appendix B.

TABLE 9

Incentives versus Changes in Labor Market Value
Estimated Coefficient (t-statistic)

Panel A: Changes in Future Salary

	Disclosure Index		*Management Forecast Accuracy*	
	CEO (1)	**CFO (2)**	**CEO (3)**	**CFO (4)**
ΔDisclosure	0.003	−0.017	−0.004	0.023
	(0.77)	(−1.58)	(−0.38)	(0.40)
HiQuality × ΔROA	0.009	0.007	−0.020	−0.618
	(0.45)	(0.08)	(−0.63)	(−2.62)
ΔROA	0.018	−0.098	0.027	0.254
	(1.13)	(−1.45)	(1.31)	(1.30)
Return	0.010***	−0.004	0.011***	−0.003
	(5.69)	(−0.93)	(3.51)	(−0.25)
HiQuality × Return	0.000	0.014**	0.003	0.010
	(0.09)	(2.20)	(0.77)	(0.65)
ΔMisses	−0.001**	0.001	0.000	−0.001
	(−1.98)	(0.45)	(0.35)	(−0.49)
ΔDeclines	0.001	−0.003	0.001	−0.002
	(1.53)	(−1.28)	(1.29)	(−0.74)
ΔLosses	0.002	−0.003	−0.001	−0.018
	(1.46)	(−0.88)	(−0.61)	(−0.91)
HiQuality × ΔMisses	0.001	0.000	−0.001	0.004
	(1.56)	(0.21)	(−0.50)	(0.74)
HiQuality × ΔDeclines	−0.001	0.003	−0.001	−0.009*
	(−1.43)	(1.02)	(−1.28)	(−1.70)
HiQuality × ΔLosses	−0.004*	−0.005	−0.000	0.007
	(−1.82)	(−0.62)	(−0.01)	(0.31)
ΔSalesGrowth	0.005	−0.026**	−0.008	−0.124*
	(0.94)	(−2.08)	(−1.03)	(−1.66)
Adjusted R^2	0.020	0.000	0.020	0.000
Number of Observations	11,379	7,605	4,834	3,337

Panel B: Partitions Based on Manager's Age Using the Disclosure Score

	CEO		CFO	
Independent Variable	**High Age (1)**	**Low Age (2)**	**High Age (3)**	**Low Age (4)**
ΔDisclosure	0.044**	0.031	0.050**	−0.004
	(2.25)	(1.59)	(2.43)	(−0.17)
HiQuality × ΔROA	−0.126	0.054	−0.204*	0.004
	(−1.61)	(0.77)	(−1.83)	(0.08)
ΔROA	0.189***	0.040	0.187**	0.091
	(2.98)	(0.72)	(2.19)	(1.41)
Return	0.064***	0.058***	0.037***	0.043***
	(6.36)	(6.34)	(3.19)	(4.25)

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TABLE 9 (continued)

Independent Variable	CEO High Age (1)	CEO Low Age (2)	CFO High Age (3)	CFO Low Age (4)
HiQuality × *Return*	0.001	0.009	−0.005	−0.007
	(0.05)	(0.80)	(−0.34)	(−0.54)
Δ*Misses*	−0.013***	−0.015***	−0.013***	−0.006*
	(−4.13)	(−4.73)	(−3.29)	(−1.78)
Δ*Declines*	−0.020***	−0.018***	−0.014***	−0.013***
	(−9.50)	(−8.11)	(−5.19)	(−5.65)
Δ*Losses*	−0.010**	−0.005	0.002	−0.004
	(−2.06)	(−1.11)	(0.36)	(−0.82)
HiQuality × Δ*Misses*	0.001	−0.007	0.003	−0.007
	(0.24)	(−1.57)	(0.50)	(−1.32)
HiQuality × Δ*Declines*	−0.000	0.001	0.004	0.006*
	(−0.03)	(0.36)	(1.12)	(1.66)
HiQuality × Δ*Losses*	−0.009	0.006	−0.021**	0.010
	(−1.12)	(0.86)	(−1.95)	(1.34)
Δ*SalesGrowth*	0.054**	0.051**	0.060	0.050*
	(2.05)	(2.22)	(1.62)	(1.84)
Adjusted R^2	0.213	0.182	0.181	0.161
Number of Observations	5,883	6,132	3,331	3,816
F-test				
Δ*Disclosure*				
F-stat	0.348		3.80*	
(p-value)	(0.55)		(0.05)	

Panel C: Partitions Based on Manager's Age Using Management Forecast Error

Independent Variable	CEO High Age (1)	CEO Low Age (2)	CFO High Age (3)	CFO Low Age (4)
Δ*Accuracy*	0.089*	0.076	0.093*	0.072
	(1.84)	(1.50)	(1.77)	(1.33)
HiAccuracy × Δ*ROA*	−0.472**	−0.097	−0.010	−0.306
	(−2.33)	(−0.50)	(−0.03)	(−1.33)
Δ*ROA*	0.586***	0.183	−0.001	0.286*
	(3.50)	(0.92)	(−0.00)	(1.69)
Return	0.066***	0.064***	0.083***	0.015
	(3.21)	(3.96)	(3.65)	(0.73)
HiAccuracy × *Return*	−0.002	−0.010	−0.046	0.011
	(−0.07)	(−0.53)	(−1.65)	(0.42)
Δ*Misses*	−0.008	−0.013**	−0.007	0.002
	(−1.27)	(−2.05)	(−1.02)	(0.33)
Δ*Declines*	−0.023***	−0.020***	−0.016***	−0.007
	(−5.39)	(−4.52)	(−3.24)	(−1.36)
Δ*Losses*	0.005	0.016	0.002	−0.014
	(0.35)	(1.44)	(0.14)	(−0.99)
HiAccuracy × Δ*Misses*	−0.014*	0.003	−0.004	−0.013
	(−1.79)	(0.38)	(−0.39)	(−1.47)

(continued on next page)

TABLE 9 (continued)

Independent Variable	CEO High Age (1)	CEO Low Age (2)	CFO High Age (3)	CFO Low Age (4)
HiAccuracy × *ΔDeclines*	0.000	0.005	0.006	−0.001
	(0.08)	(0.81)	(0.92)	(−0.15)
HiAccuracy × *ΔLosses*	−0.001	0.004	0.020	0.035
	(0.05)	(0.27)	(1.24)	(1.48)
ΔSalesGrowth	−0.033	0.055	0.161**	0.012
	(−0.71)	(1.47)	(2.23)	(0.20)
Adjusted R^2	0.233	0.170	0.140	0.113
Number of Observations	2,321	2,733	1,514	1,722
F-test				
ΔDisclosure				
F-stat	0.16		0.22	
(p-value)	(0.69)		(0.64)	

Panel D: Partitions Based on Delta of Manager's Equity Holdings Using the Disclosure Score

Independent Variable	CEO High Delta (1)	CEO Low Delta (2)	CFO High Delta (3)	CFO Low Delta (4)
ΔDisclosure	0.014	0.054***	0.007	0.065***
	(0.77)	(2.78)	(0.37)	(3.46)
HiQuality × *ΔROA*	0.025	−0.028	0.009	−0.021
	(0.32)	(−0.40)	(0.12)	(−0.28)
ΔROA	0.054	0.120**	0.057	0.090*
	(0.89)	(2.19)	(1.01)	(1.74)
Return	0.048***	0.069***	0.037***	0.042***
	(5.42)	(7.52)	(4.23)	(4.84)
HiQuality × *Return*	0.012	0.003	0.002	−0.010
	(1.02)	(0.24)	(0.20)	(−0.88)
ΔMisses	−0.011***	−0.017***	−0.009***	−0.012***
	(−3.49)	(−5.46)	(−2.89)	(−3.88)
ΔDeclines	−0.021***	−0.016***	−0.019***	−0.013***
	(−9.56)	(−8.11)	(−8.12)	(−6.83)
ΔLosses	−0.012**	−0.006	−0.005	0.002
	(−2.40)	(−1.33)	(−0.92)	(0.54)
HiQuality × *ΔMisses*	−0.003	−0.004	−0.001	−0.003
	(−0.81)	(−0.94)	(−0.31)	(−0.80)
HiQuality × *ΔDeclines*	0.003	−0.000	0.010***	0.002
	(1.01)	(−0.04)	(3.06)	(0.68)
HiQuality × *ΔLosses*	0.004	−0.001	0.003	−0.007
	(0.50)	(−0.08)	(0.35)	(−0.81)
ΔSalesGrowth	0.056**	0.051**	0.025	0.085***
	(2.48)	(2.00)	(0.96)	(3.54)
Adjusted R^2	0.182	0.204	0.171	0.175

(continued on next page)

TABLE 9 (continued)

Independent Variable	**CEO** High Delta (1)	**CEO** Low Delta (2)	**CFO** High Delta (3)	**CFO** Low Delta (4)
Number of Observations	6,123	6,123	4,469	4,470
F-test				
ΔDisclosure				
F-stat	3.92**		5.02**	
(p-value)	(0.05)		(0.03)	

Panel E: Partitions Based on Delta of Manager's Equity Holdings Using Management Forecast Accuracy

Independent Variable	**CEO** High Delta (1)	**CEO** Low Delta (2)	**CFO** High Delta (3)	**CFO** Low Delta (4)
ΔAccuracy	0.003	0.157***	0.011	0.170***
	(0.07)	(3.39)	(0.23)	(3.34)
HiAccuracy × *ΔROA*	−0.341**	−0.110	−0.292	−0.116
	(−2.23)	(−0.53)	(−1.32)	(−0.38)
ΔROA	0.483***	0.135	0.302**	0.208
	(3.64)	(0.64)	(1.98)	(0.75)
Return	0.058***	0.069***	0.044**	0.080***
	(4.03)	(3.48)	(2.45)	(3.93)
HiAccuracy × *Return*	0.004	−0.013	0.013	−0.072***
	(0.21)	(−0.56)	(0.54)	(−2.66)
ΔMisses	−0.008	−0.017**	−0.007	−0.004
	(−1.63)	(−2.52)	(−1.30)	(0.61)
ΔDeclines	−0.020***	−0.018***	−0.014***	−0.017***
	(−5.25)	(−4.73)	(−3.47)	(−3.77)
ΔLosses	0.005	−0.018	−0.012	0.001
	(0.48)	(1.26)	(−0.99)	(0.13)
HiAccuracy × *ΔMisses*	−0.008	0.001	−0.003	−0.006
	(−1.16)	(0.15)	(−0.36)	(−0.65)
HiAccuracy × *ΔDeclines*	−0.001	0.005	0.007	−0.003
	(−0.24)	(1.10)	(1.07)	(−0.53)
HiAccuracy × *ΔLosses*	0.010	−0.011	0.033*	0.022
	(0.68)	(−0.62)	(1.71)	(1.22)
ΔSalesGrowth	0.048	0.033	0.023	0.154***
	(1.36)	(0.72)	(0.41)	(2.87)
Adjusted R^2	0.225	0.190	0.122	0.185
Number of Observations	2,584	2,585	1,919	1,920
F-test				
ΔDisclosure				
F-stat	6.30**		4.20**	
(p-value)	(0.02)		(0.04)	

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TABLE 9 (continued)

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.
This table provides results from OLS regressions. In Panel A, the dependent variable is the executive's salary in year $t+1$ less the executive's salary in year t. In Panels B–E, the dependent variable is the bonus in year t scaled by total compensation in year t less the bonus in year $t-1$ scaled by total compensation for year $t-1$. In Panels B and C, the sample is split at the median of the executive's age in year t. In Panels D and E, the sample is split at the median of the delta of the executive's equity portfolio as computed in Core and Guay (1999). Industry and year fixed effects are included, but not reported. Robust standard errors are clustered at the firm level.
All other variables are defined in Appendix B.

Finally, to test whether the result differs depending on equity incentives, we follow Core and Guay (1999) and compute the delta (sensitivity of managerial wealth to changes in share price) of the executive's equity portfolio and estimate the regressions separately for samples split at the sample median. The results for the disclosure score (Panel D of Table 9) and management forecast accuracy (Panel E of Table 9) indicate that the relation between disclosure quality and bonus is stronger for the sample with weaker equity incentives. Taken together, the results favor the contention that boards link bonus payments to disclosure quality in order to encourage CEOs and CFOs to exert effort toward improving the quality of the firm's financial disclosures.

V. CONCLUSION

In this study, we examine whether boards consider effectively communicating with the investor community to be an important value-enhancing responsibility for the CEO and CFO. We provide evidence on this question by relating changes in CEO and CFO annual bonuses to changes in the quality of the firm's financial disclosures. Our first measure of disclosure quality is an aggregate measure based on analyst forecasts, and the second is management forecast accuracy. We incorporate a first-stage regression to remove the effects of environmental factors on the disclosure measures.

Overall, our evidence supports the hypothesis of a positive relation between bonuses and financial disclosure quality for both the CEO and CFO. We also find that the relation between disclosure quality and CEO/CFO compensation is stronger for high-growth firms and for firms with stronger governance structures. The former result is consistent with firms providing a greater reward for financial disclosure quality when the market demand for high-quality financial information is greater, and the latter result is consistent with higher-quality boards being more likely to recognize the importance of financial disclosure quality and incorporating it into the firm's incentive structure.

To provide additional confidence that our findings are not driven by firm-specific correlated omitted variables, we examine corresponding relations for the COO, an officer who is not responsible for investor relations. For the COO, we do not find a significant relation between disclosure quality and bonus payments. To provide additional assurance that forecast accuracy does not proxy for financial performance, we find that the management forecast results hold when actual earnings exceed the forecast. We also conduct a series of tests to investigate whether the observed positive relation between disclosure quality and bonus payments is driven by the managerial labor market using disclosure quality as a signal of the manager's ability and adjusting the manager's reservation wage accordingly. The results support the agency theory explanation over the reservation adjustment explanation.

Overall, our findings suggest that boards consider financial disclosure quality to be an important responsibility and signal of ability for both the CEO and the CFO. As a result, they implicitly incorporate financial disclosure quality when determining bonuses of CEOs and CFOs.

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APPENDIX A

Examples of Proxy Statements on CEO Responsibility

1. Dominion Resources, Inc. 2009

"Mr. Farrell participates in the same compensation programs and receives compensation based on the same philosophy and factors as other NEOs. Application of the same philosophy and factors to Mr. Farrell's position results in overall CEO compensation that is significantly higher than the compensation of the other NEOs. His compensation is commensurate with his greater responsibilities and decision-making authority, broader scope of duties that encompasses the entirety of the company (as compared to the other NEOs who are responsible for significant but distinct areas within the company) and his overall responsibility for corporate strategy. *His compensation also reflects his role as our primary corporate representative to investors, customers, regulators, analysts, legislators, industry and the media.*"

2. Wells Fargo & Company 2008

"For 2008, the individual qualitative objectives for our CEO and for our Chairman consisted of the following:

For Mr. Stumpf, as CEO—

- Continue to develop the leaders for the future through career development, succession planning and mentoring;
- Reinforce the culture and vision and values of Wells Fargo through a challenging economic environment;
- Invest in the business in an effective manner with available resources, including investments in technology to improve our service reliability, especially in the mainframe environment;
- Set expectations and hold leadership accountable for the expense initiative results;
- *Continue to promote and represent the Company to our investors and the analyst community;* and
- Continue to develop strong, open relationships with the Board."

3. FedEx Corporation 2009

"Chairman of the Board, President and Chief Executive Officer. Mr. Smith's AIC payout is tied to the achievement of our internal goals for company financial performance for the fiscal year.

- FedEx's stock price performance relative to the Standard & Poor's 500 Composite Index, the Dow Jones Transportation Average, the Dow Jones Industrial Average and competitors;
- FedEx's stock price to earnings (P/E) ratio relative to the Standard & Poor's 500 Composite Index, the Dow Jones Industrial Average and competitors;
- *Analyst coverage and ratings for FedEx's stock;*
- FedEx's U.S. and international revenue market share; and
- *FedEx's reputation rankings by various publications and surveys.*"

4. Entergy Corporation 2009

"In December 2008, the Committee set the 2009 target award for incentives to be paid in 2010 under the Annual Incentive Plan for our Chief Executive Officer at 120 percent of his base salary and the target awards for each other Named Executive Officer at 70 percent of their respective base salaries.

The Personnel Committee's assessment of Mr. Leonard's strong performance based on the Board's annual performance evaluation, in which the Board reviews and assesses Mr. Leonard's performance based on: leadership, strategic planning, financial results, succession planning, *communications with all of our stakeholders, external relations with the communities and industries in which we operate and his relationship with the Board.*"

APPENDIX B
Variable Definitions

Panel A: First-Stage Regressions

Variable Name	Definition
Disclosure Index	The sum of decile ranking each year of analyst forecast error, forecast dispersion, and revision volatility, with the highest decile representing the lowest error, lowest dispersion, and lowest volatility divided by 30.
Forecast Error	The absolute difference between the last consensus forecast of EPS estimate prior to the release of earnings and the I/B/E/S earnings per share scaled by the beginning-of-year stock price.
Forecast Dispersion	The standard deviation of the analyst forecasts included in the year-end consensus forecast deflated by the absolute value of the mean consensus forecast at the end of the year.
Revision Volatility	The standard deviation of the monthly revision of the median forecast deflated by the beginning-of-year price.
Management Forecast Accuracy	The absolute difference of actual earnings and management forecast EPS divided by the beginning-of-year stock price multiplied by −1.
LnAssets	Log of total assets as of the beginning of year *t*.
Market-to-Book	Market value of equity divided by the book value of equity at the beginning of year *t*.
ROA	Earnings before extraordinary items divided by beginning total assets.
Institutions	Number of shares held by institutions divided by beginning total outstanding common shares.
Leverage	Book value of total liabilities divided by the book value of assets.
Loss	Indicator variable set equal to 1 if the firm recorded a loss, and 0 otherwise.
ROA Volatility	Standard deviation of the annual return on assets for the six-year period immediately prior to the current year.
Analyst Coverage	The number of analysts following the firm as of the beginning of year *t*.
Foreign Sales	Total foreign sales divided by beginning total assets.
Insider Ownership	The number of common shares owned by officers and directors divided by total outstanding common shares as of the beginning of year *t*.
R&D	Research and development expenses divided by beginning total assets.

(continued on next page)

APPENDIX B (continued)

Variable Name	Definition
Segments	Log of the number of business segments.
Shareholders	Number of common shareholders (in thousands) as of the beginning of year *t*.

Panel B: Compensation Regressions

Variable Name	Definition
ΔBonus	The annual bonus in year *t* scaled by total compensation for year *t* less the annual bonus in year *t*−1 scaled by total compensation in year *t*−1.
ΔDisclosure	The change in disclosure quality not explained by environmental factors, calculated as the residual from the regression in Equation (1) for year *t* minus that for year *t*−1.
ΔAccuracy	The change of residual from the regression on management forecast accuracy in Equation (1) for year *t* minus that for year *t*−1.
HiQuality	Equal to 1 if *ΔDisclosure* is above the sample median, and 0 otherwise.
HiAccuracy	Equal to 1 if *ΔAccuracy* is above the sample median, and 0 otherwise.
ΔROA	The return on assets for year *t* less the return on assets for year *t*−1.
ΔSalesGrowth	The change of sales growth of year *t* over year *t*−1, where sales growth of year *t* is measured over three years, from *t*−1 to *t*+1.
Return	The annualized stock return for year *t*.
ΔMisses	The number of quarters the firm missed the consensus analyst forecast during the year less the number of quarters missed in the prior year.
ΔDeclines	The number of quarters the firm reported EPS below the EPS for the same quarter of the preceding year less the number of quarters missed in the prior year.
ΔLosses	The number of quarters in which the firm reported earnings below zero less the number of quarters below zero in the prior year.
Sales Growth	Sales growth over three years, from *t*−1 to *t*+1.
Governance	The standardized factor of the percentage of outsiders on the board, number of board meetings, and board size.

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Analyst Interest as an Early Indicator of Firm Fundamental Changes and Stock Returns

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ABSTRACT: We posit that a change in analyst interest in a firm is an early indicator of the firm's future fundamentals, capital market activities, and stock returns. We measure increases in analyst interest by obser.ving analysts who do not cover a firm but participate in that firm's earnings conference call, and we measure decreases in analyst interest by observing analysts who cover a firm, yet are absent from that firm's call. We find that increases in analyst interest are positively associated with future changes in firm fundamentals and capital market activities, while decreases in analyst interest are negatively associated with capital market activities. We also find that increases (decreases) in analyst interest are positively (negatively) correlated with future stock returns over the next three months and that a hedge portfolio yields a significant abnormal return. Overall, our study shows that analyst interest is a novel and early indicator of future firm fundamentals and capital market consequences.

Keywords: *analyst interest; firm fundamentals; analyst coverage; institutional ownership; trading volume; stock returns.*

JEL Classifications: *G11; G12; G14; G31; M41.*

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I. INTRODUCTION

A large literature examines the link between firm fundamentals and future stock returns (e.g., Ou and Penman 1989; Bernard and Thomas 1990; Holthausen and Larcker 1992; Sloan 1996; Abarbanell and Bushee 1997, 1998; Piotroski 2000). Typically, the motivation for this line of research is that firm fundamentals are reflected in accounting data, which are informative about a firm's future cash flows, and that investors do not fully impound this information into stock prices. But since financial statement information is backward-looking, it is beneficial for investors to identify early indicators of firm fundamental changes that are not yet reflected in financial statements. In this paper, we examine whether a change in analyst interest, proxied by either the onset of non-covering sell-side equity analysts who participate in a firm's earnings conference call or by the absence of participation from covering analysts, is an early indicator of not only firm fundamental changes, but also of future capital market activities and stock returns.

Our focus on analyst interest stems from two observations. First, prior research shows that analysts are sophisticated experts with deep industry knowledge (Mikhail, Walther, and Willis 1999; Asquith, Mikhail, and Au 2005; Kadan, Madureira, Wang, and Zach 2012). Based on their knowledge of new products, customers, and market opportunities within an industry, analysts are keenly aware of shifting competitive positions between industry leaders, laggards, up-and-comers, and new entrant firms. Their expertise allows them to foresee future changes in firms' fundamentals, well before such information is reflected in the financial statements of individual firms. Second, before an analyst initiates coverage on a firm, he or she must conduct due diligence. Analyst due diligence has not been explored in the extant literature, as many of the activities analysts perform before and after coverage initiations have been described as a black box (Ramnath, Rock, and Shane 2008; Bradshaw 2011). We explore one aspect of analyst due diligence and document that analysts regularly participate in a firm's earnings conference calls *before* initiating coverage on the firm.[1] This practice is common because listening to and asking a question during a firm's conference call is part of an extensive, and sometimes lengthy, due diligence process. For example, Sanford C. Bernstein & Co., a top-ranked sell-side equity research firm in Institutional Investor's annual All-American Research Survey, gives newly hired analysts up to one year to conduct due diligence on firms before initiating coverage on them (Koo 2012). In a similar vein, analysts who already cover a firm are often absent from, or silent on (do not ask a question), the firm's conference call if they have lost interest in the firm or intend to drop coverage.[2]

We posit that an onset of analysts who do not cover a given firm ("non-covering analysts"), but participate in that firm's earnings conference call, captures increasing analyst interest in the firm, while analyst absenteeism captures decreasing analyst interest in the firm. A change in analyst

[1] Two examples come from our data. First, Asset Acceptance Capital Corp held its first-ever earnings conference call on March 10, 2004. But as of that date, no sell-side analysts had initiated coverage on the firm. Yet, during the question-and-answer (Q&A) portion of the call, six people asked management a question. According to the transcript, three of the questioners were sell-side analysts, one was a buy-side analyst, and two provided no employer affiliation. After that call, two of the sell-side analysts subsequently initiated coverage on the firm, one on March 16 (Buy rating) and the other on April 2 (Outperform rating). A second example is from Bebe Stores Inc., which had seven sell-side analysts participate in its April 22, 2002 earnings conference call, six of which officially covered the firm. The one analyst who was not yet covering the firm subsequently initiated coverage on May 1, 2002 (Market Perform rating).

[2] For example, Morgan Stanley discontinued coverage on Computer Sciences Corp. on March 23, 2006 (rating from Underweight to N/A). The Morgan Stanley analyst participated in the earnings conference call on November 3, 2005, but not the one on February 7, 2006. We note that we cannot distinguish between cases in which an analyst did not dial into a conference call (i.e., absent) or dialed into a call, but did not ask a question (i.e., silent). Any misclassification on our part would add measurement error to our empirical proxy for covering analysts who are absent (explained further in Section III).

interest serves as an early indicator of a change in future firm fundamentals, capital market activities, and stock returns. Our proposition stems from two non-mutually exclusive theories. The first theory is from McNichols and O'Brien (1997), who show that analysts allocate effort toward firms in which they view future prospects to be favorable and curtail effort in firms with poor prospects or whose coverage is likely to be discontinued.[3] This theory suggests that analyst interest—our measure of analyst effort prior to coverage initiation or termination—predicts future reported firm fundamentals, analyst coverage, and stock returns. The second theory is from Merton (1987), who shows that investor recognition of a firm affects the firm's cost of capital and stock price. This theory suggests that a change in analyst interest leads to a change in investor recognition of a firm (i.e., holdings and trading), especially among institutional investors, given their broker-client relationships with sell-side analysts, as well as a change in stock price.

While most indicators of an analyst's early interest or disinterest in a firm are unobservable, conference call participation and absenteeism are observable through available transcripts. Conceptually, our measure of increasing analyst interest is based on an observable aspect of analysts' due diligence prior to their formal issuance of earnings forecasts, price targets, and stock recommendations to the public. Similarly, our measure of decreasing analyst interest is based on the observable lack of participation by a covering analyst during the conference call. Our analyst interest measures have two appealing practical attributes: almost all questioners are sell-side equity analysts, enabling us to capture the interest of sophisticated experts,[4] and virtually all public firms hold quarterly earnings conference calls, allowing for a large sample of firms with variation in size and existing levels of analyst following.

Using a sample of conference call transcripts from 2002 through the first quarter of 2009, we create two measures to capture analysts' early interest and disinterest in a firm. We define *NC_ANALYSTS* as the number of non-covering analysts who ask a question during the conference call, and *COV_ANALYSTS_ABSENT* as the number of covering analysts who asked a question during the prior quarter's conference call, but are absent (or silent) during the current conference call, both scaled by the total number of callers on the current conference call.[5] We find that *NC_ANALYSTS* is positively associated with proxies for future changes in firm fundamentals, earnings per share (EPS), and sales growth, up to four quarters ahead, after controlling for other factors. We also find that *NC_ANALYSTS* is positively associated with a change in the next quarter's analyst coverage, institutional ownership, and trading volume. The results are consistent with our prediction that an increase in analyst interest in a firm is an early indicator of improvement in future reported firm fundamentals and capital market activities in the stock. Finally, we find that *NC_ANALYSTS* predicts future stock returns, over and above the firm's earnings surprise, size, the book-to-market ratio, and the past 11-month return, and after controlling for the aforementioned changes in capital market activities. Subsequent three-month stock returns increase monotonically from 1.69 percent in the bottom *NC_ANALYSTS* quartile to 3.56 percent in the top quartile, resulting in a significant hedge portfolio return of 1.87 percent. After controlling for common return factors, the hedge portfolio yields a significant abnormal return of 0.475 percent per month or 5.7 percent per year. The magnitude of the hedge return is

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[3] Consistent with this notion, Ertimur, Muslu, and Zhang (2011) document that coverage initiations are mostly started with a Buy rating. In particular, about 68 percent of initiations are started with a Strong Buy or Buy rating, compared to 3 percent of initiations started with a Strong Sell or Sell rating.

[4] We find that over 92 percent of the questioners on earnings conference calls are affiliated with a sell-side brokerage firm. The remaining questioners are either institutional investors (buy-side analysts) or not identifiable due to a vague or incomplete name or affiliation.

[5] We require covering analysts to have been on the previous conference call to distinguish them from covering analysts who never participate in a firm's conference call.

economically significant, especially given the fact that many trading strategies have not worked well in the past ten years (Green, Hand, and Soliman 2011).[6]

In contrast, *COV_ANALYSTS_ABSENT* exhibits no statistical association with proxies for firm fundamental changes or trading volume. However, it is significantly and negatively related to a change in the next quarter's analyst coverage and institutional ownership. These findings provide some evidence that a drop in analyst interest is an early indicator of a decrease in capital market activities. We also find that *COV_ANALYSTS_ABSENT* predicts future stock returns. Subsequent three-month stock returns decrease from 3.15 percent in the bottom *COV_ANALYSTS_ABSENT* quartile to 1.48 percent in the top quartile, resulting in a statistically significant return difference of -1.67 percent.

We rule out a number of alternative explanations for our results, including confounding information, an upward trend in conference call data, microstructure effects, and investor overreaction. We also conduct several additional tests and robustness checks. We partition our sample into three groups based on the level of existing analyst coverage, and find the effects of analyst interest to be more pronounced for low-coverage ("neglected") firms. We also find that our results are robust to alternative specifications of the analyst interest variables. Finally, we show that our results are not driven by initial public offering (IPO) firms or fourth-quarter observations.

This study contributes to the extant literature in three unique ways. First, it adds to the literature examining the link between firm fundamentals and future stock returns. Since financial statements are backward-looking, accounting information may not be timely with respect to changes in firm fundamentals. We show that an awareness of changing analyst interest in a firm can provide investors with an early indicator of a firm's fundamental changes that are not yet reflected in its financial statements. Second, our study contributes to the literature on sell-side analysts by highlighting one aspect of their due diligence process prior to their formal issuance of earnings forecasts, price targets, and stock recommendations to the public and, hence, adds to our understanding of the role analysts play in the capital markets. Our measure of increasing analyst interest, based on pre-coverage due diligence, also distinguishes our study from the prior literature on analyst discrimination (e.g., Mayew 2008; Cohen, Lou, and Malloy 2014) because the views of the non-covering analysts are not yet known to the firm managers prior to the conference calls (at least in terms of a published rating on the stock). Finally, our study adds to prior studies documenting the information content of conference calls and their effects on analysts covering the firms (Frankel, Johnson, and Skinner 1999; Bowen, Davis, and Matsumoto 2002; Bushee, Matsumoto, and Miller 2003; Kimbrough 2005). In particular, we highlight that analyst participation or absenteeism can be a measure of analyst interest in a firm and, moreover, is informative about their future coverage decisions.

This paper continues as follows. The next section develops testable hypotheses. Section III describes the sample and variable construction. Section IV presents the empirical findings. Section V discusses alternative explanations and robustness tests. We conclude in Section VI.

II. HYPOTHESES DEVELOPMENT

Prior studies examine extensively the link between accounting data and future stock returns. Bernard and Thomas (1989, 1990) find that investors do not fully understand the implications of current earnings for future earnings, leading to predictable return drifts in the four quarters subsequent to earnings announcements. Similarly, Sloan (1996) shows that investors do not understand the differential implications of the accrual and cash flow components of current earnings for future earnings. Thus, a hedge portfolio based on extreme deciles of accruals earns significant

[6] Plenty of anecdotal evidence suggests poor performance for quantitative-based trading strategies. For example, Goldman Sachs closed its Global Alpha hedge fund, which relied on computer-driven trading strategies, in 2011.

abnormal returns. Ou and Penman (1989) and Abarbanell and Bushee (1997) show that fundamental signals constructed from accounting data have predictive power for future earnings and, thus, predict future stock returns. Similarly, Holthausen and Larcker (1992) and Lev and Thiagarajan (1993) find that accounting-based fundamental signals are value-relevant over contemporaneous earnings. Finally, Abarbanell and Bushee (1998) find that a trading strategy based on these fundamental signals generates an average 12-month cumulative size-adjusted return of 13.2 percent, suggesting that contemporaneous stock returns do not fully reflect the implications of the fundamental signals for future earnings. In sum, this literature establishes that predicting firm fundamentals is central to the analysis and valuation of stocks.

One of the most important roles for sell-side equity analysts is predicting firms' future fundamentals and stock valuations (Bradshaw 2011; Brown, Call, Clement, and Sharp 2015). Given their deep industry knowledge, analysts are aware of firms' shifting competitive positions well before such information is reflected in the financial statements and stock prices of individual firms. Accordingly, their fundamental analyses and predictions published in written reports have been shown to be informative to the markets (Mikhail et al. 1999; Asquith et al. 2005; Kadan et al. 2012). We exploit two important institutional regularities within the analyst coverage process to develop an early indicator of a firm's prospects. First, before an analyst initiates coverage on a firm, he or she requires a certain amount of time and effort to become informed about the firm (i.e., conduct due diligence). The amount of time needed to complete due diligence varies from a minimum of several weeks to one year.[7] Since earnings conference calls occur every quarter, there is ample opportunity for an analyst to participate in a firm's call before initiating coverage on the firm. Second, while most analysts who already cover a firm actively participate in the firm's conference call each quarter, it is not uncommon for some analysts to be silent or possibly absent from the call. We posit that such absence results from a loss of interest in the firm and a possible intention to drop coverage. Taken together, these two scenarios within the analyst coverage process can provide an early indication about a firm's fundamentals and prospects.

Our proposition that changes in analyst interest in a firm predict future fundamental changes, capital market activities, and stock returns is based upon two non-mutually exclusive theories. The first is from McNichols and O'Brien (1997), who show that analysts allocate effort toward firms in which they view future prospects to be favorable. The fact that over two-thirds of analyst coverage initiations are started with a Buy or Strong Buy rating (Ertimur et al. 2011) is consistent with analysts having exerted effort to learn about and initiate coverage on firms in which the positive fundamentals are not yet reflected in financial statements or stock prices. The findings in McNichols and O'Brien (1997) also indicate that analysts curtail effort toward firms with poor prospects or whose coverage is likely to be discontinued. This theory suggests that analyst interest—our measure of analyst effort prior to coverage initiation or termination—should predict future firm fundamentals, analyst coverage, and stock returns. The second theory is from Merton (1987), who shows that investor recognition of a firm affects the firm's cost of capital and stock price. This theory suggests that a change in analyst interest should lead to a change in investor recognition of a firm (i.e., holdings and trading), especially among institutional investors, given their broker-client relationships with sell-side analysts, and a change in stock price.[8]

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[7] Analysts vary in their speed to initiate coverage on firms. Our conversations with a number of sell-side analysts indicate that a lower bound of three weeks is not unreasonable. Due diligence tasks include analyzing past financial statements, preparing models and forecasts, visiting company sites, meeting management, listening to archived conference calls, drafting and editing an initiation report, and receiving approval from the brokerage firm's research executive management. The upper bound of 365 days is based on anecdotes of Sanford C. Bernstein & Co. allowing newly hired analysts up to one year to learn about a firm prior to initiation of coverage (Koo 2012).

[8] Lehavy and Sloan (2008), Da, Engelberg, and Gao (2011), and Drake, Roulstone, and Thornock (2012) document evidence consistent with the prediction of Merton's (1987) investor recognition story.

Of the two theories, only the one from McNichols and O'Brien (1997) suggests an association between early analyst interest in a firm and that firm's future fundamentals. This difference provides us with one prediction by which to distinguish between these two theories. If analysts exert effort to learn about firms with positive prospects, then analyst participation in a firm's earnings conference call prior to coverage initiation should capture analysts' pre-initiation effort and positive views about that firm. Such views can eventually be discussed in the analysts' written initiation reports and reflected in their forecasts of sales and earnings. Analogously, if analysts foresee negative prospects for a firm that is already covered and lose interest in participating in that firm's earnings conference call, then the lack of participation can indicate a possible future downgrade or termination of coverage. In sum, we predict that increases in analyst interest are associated with future improvement in firm fundamentals, while decreases in analyst interest are associated with future deterioration in firm fundamentals:

Prediction 1: Increases (decreases) in analyst interest are associated with future improvement (deterioration) in firm fundamentals.

In contrast to the first prediction, a second prediction suggesting that early analyst interest is associated with subsequent capital market activities is supported by both theories. Under the McNichols and O'Brien (1997) theory, some of the non-covering analysts who participate in a firm's conference calls due to expectations of improving fundamentals will eventually initiate coverage on the firm. Increased analyst coverage, especially with positive recommendations, will attract more institutional investors and institutional trading in the stock. Under the Merton (1987) theory, more institutional investors will become informed about a firm's prospects through conversations with the analysts. With increased interest from institutional investors, future trading volume also increases. These interactions between analysts and investors suggest that increases (decreases) in analyst interest are associated with future increases (decreases) in capital market activities, such as analyst coverage, institutional ownership, and trading volume:

Prediction 2: Increases (decreases) in analyst interest are associated with future increases (decreases) in analyst coverage, institutional ownership, and trading volume.

Finally, both theories suggest that analyst interest predicts future stock returns. Under the McNichols and O'Brien (1997) theory, analysts follow firms with positive prospects, and future stock prices will reflect the future improvement in firm fundamentals. Under the Merton (1987) theory, higher analyst interest in a firm leads to greater recognition from institutional investors, which will lead to lower cost of capital and higher demand and valuation for the firm's stock. Conversely, a loss of interest from analysts will be reflective of poor prospects for a firm, and a loss of interest from institutional investors will lead to a decline in demand and valuation for a firm's stock:

Prediction 3: Increases (decreases) in analyst interest are positively (negatively) associated with future stock returns.

In summary, we posit that increases (decreases) in analyst interest capture improvements (deterioration) in firm fundamentals that are not yet reflected in financial statements. Thus, changes in analyst interest serve as an early indicator of future changes in *reported* firm fundamentals, capital market activities, and stock returns.

III. DATA AND VARIABLE DEFINITIONS

Our data are comprised of firms with available conference call transcripts from the Thomson Reuters StreetEvents database from the first quarter of 2002 through the first quarter of 2009. The

transcripts contain identifying information about the firm managers on the call, as well as the name and affiliation of anyone who asked a question during the question-and-answer (Q&A) portion of the call.[9] There are transcripts from many types of conference calls, including calls about technology announcements, sales and marketing initiatives, mergers and acquisitions, restructurings, and earnings announcements. However, only earnings calls are scheduled far in advance, with analysts well aware of the exact date and time of the call, making conference call participation a logical measure of analyst interest. In addition, many of the non-earnings calls do not have a Q&A portion. Therefore, we use only the transcripts of quarterly earnings conference calls of U.S. firms, resulting in a sample of 55,565 conference calls from 3,370 firms.[10] Table 1, Panel A provides a breakdown of the sample conference calls by year and quarter.[11]

From each firm's conference call transcript, we identify those on the call who are sell-side equity analysts using two procedures. First, a caller is identified as an analyst if the last name, first initial, and brokerage affiliation match the equivalent information contained in the I/B/E/S Detail Recommendations file.[12] For cases in which there is no match, we check for possible misspellings of the names and affiliations on the transcripts and manually identify the callers as analysts when it is obvious that the initial mismatch was due to a simple misspelling. With this procedure, we identify 80.5 percent of the callers as I/B/E/S sell-side analysts. Second, callers are also identified as sell-side analysts if their affiliation is a brokerage firm that does not report to I/B/E/S, such as Merrill Lynch, Lehman Brothers, BB&T Capital Markets, Wachovia, and SG Cowen. We identify 11.7 percent of the callers as non-I/B/E/S sell-side analysts. The remaining 7.8 percent of callers are either institutional investors or buy-side analysts (based on their affiliation) or not identifiable due to a vague or incomplete name or affiliation.[13]

For each caller classified as a sell-side equity analyst tracked by I/B/E/S, we obtain the unique analyst code used by I/B/E/S to identify that analyst's earnings estimates or recommendations for a given firm.[14] We then determine whether the analyst on the earnings conference call has initiated coverage on the firm prior to the date of the conference call. Specifically, if an analyst is on a firm's earnings conference call, but has not yet issued any earnings estimates or recommendations at any time during the 12 months prior to the call, then we classify that analyst as a non-covering analyst. All other analysts have issued earnings estimates or recommendations prior to the conference call

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[9] A better measure of analyst interest in a firm would be the number of analysts who dial into and listen to the firm's earnings conference call. However, such information is not available on transcripts. Therefore, the number of analysts who ask a question is the next-best alternative.

[10] We require that the date of a firm's conference call (from Thomson Reuters) be the same or one day after the date of the earnings announcement provided by Compustat. We find that 78 percent of the conference calls occur on the same date as the earnings announcement and 22 percent occur on the next day.

[11] Any firm that hosts an earnings conference call in which no analysts ask a question is excluded from our sample. Such exclusions are rare, as we find that 97.5 percent of earnings conference call transcripts for U.S. firms in the StreetEvents database from the first quarter of 2002 through the first quarter of 2009 are included in our final sample (55,565 out of 56,994 conference calls).

[12] In the I/B/E/S Detail Recommendations file, an analyst's last name and first initial are identified in the variable called ANALYST and the brokerage affiliation is identified in a variable called ESTIMID. In rare cases in which a brokerage firm employs two analysts with the same last name and first initial, we distinguish the correct analyst by additionally matching on firm or industry. That is, the analyst must have covered the firm on the transcript (i.e., made a recommendation) either before or after the conference call date, or must have at least covered a firm in the same two-digit Standard Industrial Classification (SIC) code as the firm on the transcript.

[13] The latter group includes callers who cannot be matched to an analyst in the I/B/E/S file, even after checking for misspellings of the name and brokerage affiliation. The participation of these analysts in a firm's conference call is very rare, and our results are virtually unchanged if we include these analysts in the sample.

[14] A given analyst's unique identifier in I/B/E/S is a six-digit number labeled as AMASKCD in the Detail Recommendations file and as ANALYS in the Detail History Earnings per Share file. Although the identifier is labeled as two different variable names in the two files, it is the same number for a given analyst.

TABLE 1

Sample and Summary Statistics

Panel A: Conference Calls by Year and Calendar Quarter

Year	1st Qtr.	2nd Qtr.	3rd Qtr.	4th Qtr.	Total
2002	119	495	654	1,142	2,410
2003	1,325	1,321	1,564	1,724	5,934
2004	1,781	1,734	1,829	1,880	7,224
2005	1,949	2,022	2,077	2,128	8,176
2006	2,156	2,156	2,294	2,299	8,905
2007	2,328	2,366	2,455	2,545	9,694
2008	2,642	2,669	2,671	2,641	10,623
2009	2,599	—	—	—	2,599
Total	14,899	12,763	13,544	14,359	55,565

Panel B: Descriptive Statistics

Variable	n	n Missing	Mean	1st Quartile	Median	3rd Quartile
NUMCALLERS	55,565	—	5.09	3.00	5.00	7.00
NC_ANALYSTS (unscaled)	55,565	—	1.06	0.00	1.00	2.00
COV_ANALYSTS (unscaled)	55,565	—	4.04	2.00	4.00	6.00
COV_ANALYSTS_ABSENT (unscaled)	52,195	3,370	1.24	0.00	1.00	2.00
NC_ANALYSTS (scaled)	55,565	—	0.25	0.00	0.17	0.38
COV_ANALYSTS (scaled)	55,565	—	0.75	0.63	0.83	1.00
COV_ANALYSTS_ABSENT (scaled)	52,195	3,370	0.30	0.00	0.17	0.40
ΔEPS_{t+1}	53,990	1,575	−0.01	−0.01	0.00	0.01
$SGROWTH_{t+1}$	51,436	4,129	0.19	−0.01	0.10	0.23
NAN	55,565	—	7.90	3.00	6.00	11.00
$CNAN_{t+1}$ (unscaled)	55,565	—	0.16	−1.00	0.00	1.00
$CNAN_{t+1}$ (percentage change)	52,826	2,739	0.05	−0.06	0.00	0.13
NII	55,565	—	166.53	66.00	119.00	202.00
$CNII_{t+1}$ (unscaled)	55,565	—	1.77	−5.00	1.00	8.00
$CNII_{t+1}$ (percentage change)	53,198	2,367	0.02	−0.05	0.01	0.07
$CTURNOVER_{t+1}$	54,434	1,131	0.04	−0.11	0.02	0.18
RET	54,040	1,525	0.02	−0.11	0.01	0.13
SIZE	54,085	1,480	6.94	5.83	6.83	7.96
ROA	54,130	1,435	0.00	0.00	0.01	0.02
BTM	52,690	2,875	0.54	0.28	0.45	0.67
LEVERAGE	54,085	1,480	0.53	0.01	0.17	0.52
PASTRET	54,370	1,195	0.00	−0.10	−0.01	0.09
VOLATILITY	54,369	1,196	0.02	0.01	0.02	0.03

(continued on next page)

and are classified as covering analysts. Since we cannot determine the coverage status for non-I/B/E/S analysts, we exclude them from our analysis.

For each conference call, we define *NC_ANALYSTS* as the number of non-covering analysts and *COV_ANALYSTS* as the number of covering analysts, both scaled by the total number of callers (*NUMCALLERS*) who appear (i.e., ask a question) on the conference call transcript. We also define

TABLE 1 (continued)

Panel C: Selected Pair-Wise Correlation Variables $NC\_ANALYSTS_t$ to $CTURNOVER_{t+1}$

	1	2	3	4	5	6
1 $NC\_ANALYSTS_t$						
2 $COV\_ANALYSTS\_ABSENT_t$	−0.03***					
3 ΔEPS_{t+1}	0.02***	0.00				
4 $SGROWTH_{t+1}$	0.02***	−0.01***	0.10***			
5 $CNAN_{t+1}$	0.06***	−0.09***	0.02***	0.05***		
6 $CNII_{t+1}$	0.04***	−0.04***	0.08***	0.10***	0.11***	
7 $CTURNOVER_{t+1}$	0.02***	−0.01*	−0.03***	0.00	0.03***	0.12***
8 $SIZE_t$	−0.30***	0.06***	0.03***	−0.03***	0.00	−0.01***
9 ROA_t	−0.07***	−0.03***	0.02***	−0.07***	0.05***	0.07***
10 BTM_t	0.11***	0.02***	−0.18***	−0.12***	−0.09***	−0.12***
11 $LEVERAGE_t$	0.05***	0.02***	−0.07***	−0.04***	−0.05***	−0.06***
12 $PASTRET_t$	0.01	−0.03***	0.10***	0.06***	0.07***	0.33***
13 $VOLATILITY_t$	0.12***	0.02***	−0.10***	0.02***	−0.06***	−0.08***

Panel D: Selected Pair-Wise Correlation Variables $SIZE_t$ to $VOLATILITY_t$

	7	8	9	10	11	12
8 $SIZE_t$	0.02***					
9 ROA_t	0.02***	0.25***				
10 BTM_t	−0.01	−0.29***	−0.13***			
11 $LEVERAGE_t$	0.01**	−0.06***	−0.06***	0.45***		
12 $PASTRET_t$	0.05***	0.07***	0.09***	−0.15***	−0.09***	
13 $VOLATILITY_t$	−0.12***	−0.48***	−0.32***	0.36***	0.23***	−0.08***

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a two-tailed test.
Panel A presents the sample of earnings conference call transcripts by year and calendar quarter. Panel B presents descriptive statistics of variables for the entire conference call transcript sample. Panels C and D present Pearson pair-wise correlations.
Variables not shown below are defined in Appendix A.

Variable Definitions:
NUMCALLERS = number of analysts that asked a question on the firm's conference call;
NC_ANALYSTS = number of analysts that asked a question on the firm's conference call, but did not cover the firm as of the date of the conference call, scaled by *NUMCALLERS*;
COV_ANALYSTS_ABSENT = number of analysts that covered the firm as of the date of the conference call, did not ask a question on the conference call, but did ask a question on the previous conference call, scaled by *NUMCALLERS*;
NAN = number of analysts that covered the firm as of the date of the conference call;
$CNAN_{t+1}$ = percentage change in the number of analysts that covered the firm;
NII = number of institutional investors that owned the firm's stock as of the most recent calendar quarter ended prior to the conference call;
$CNII_{t+1}$ = percentage change in *NII*;
$CTURNOVER_{t+1}$ = change in average daily turnover;
ΔEPS_{t+1} = seasonal difference in earnings per share;
$SGROWTH_{t+1}$ = seasonal percentage change in quarterly sales for the fiscal quarter ended after the conference call;
SIZE = natural logarithm of the market value of equity;
ROA = income before extraordinary items divided by total assets;
BTM = book value of equity divided by market value of equity;
LEVERAGE = book value of debt divided by book value of equity;
PASTRET = size-adjusted past return; and
VOLATILITY = standard deviation of daily size-adjusted returns.

COV_ANALYSTS_ABSENT as the number of covering analysts who were on the prior quarter's conference call, but are absent from, or silent on, the current conference call, scaled by *NUMCALLERS*.[15] Descriptive statistics of these variables are provided in Table 1, Panel B. The mean number of callers is 5.1, of which 1.1 are non-covering analysts and 4.0 are covering analysts. The mean values of *NC_ANALYSTS* and *COV_ANALYSTS* are 0.25 and 0.75, respectively. Unscaled *COV_ANALYSTS_ABSENT* has a mean value of 1.2 and a scaled mean value of 0.30.

We note several sources of measurement error (i.e., noise) in our variables of interest. *NC_ANALYSTS* only captures non-covering analysts who ask a question, so to the extent that there are non-covering analysts who listen to a call, but do not ask a question, our measure of increasing analyst interest will be understated. A similar measurement error exists with *COV_ANALYSTS_ABSENT*. Although we require a covering analyst to have asked a question during the prior conference call (to rule out analysts who never ask a question), that analyst may not be absent on the current conference call, but instead simply does not ask a question, in which case, our measure of decreasing analyst interest will be overstated. Most importantly, while a lengthy analyst due diligence process suggests that an analyst is likely to participate in a firm's conference call before initiating coverage, it does not suggest that an analyst would be absent from a conference call before dropping coverage. To the extent that covering analysts drop coverage before they stop participating in a given firm's conference calls, *COV_ANALYSTS_ABSENT* will be understated in measuring decreasing analyst interest. Therefore, we expect relatively more noise in *COV_ANALYSTS_ABSENT*, which tends to attenuate the coefficients toward zero more so than the coefficients on *NC_ANALYSTS* in our regressions if the noise is uncorrelated with the other independent variables.[16]

To measure firm fundamentals, capital market activities, and future stock returns, we obtain financial statement data from Compustat, stock data from CRSP, institutional holdings data from the Thomson Reuters Form 13F database, and earnings estimates from I/B/E/S. We choose EPS growth and sales growth to proxy for firm fundamentals because they are arguably the two most emphasized metrics in firms' quarterly earnings announcements. Also, analysts typically forecast EPS and sales, and the most common valuation models used by analysts are based on EPS (P/E [price/earnings]) and EPS growth (PEG [price/earnings to growth]) (Bradshaw 2004; Brown et al. 2015). We measure EPS growth (ΔEPS_x) as the seasonal difference in diluted EPS excluding extraordinary items, measured one to four quarters after the conference call ($x = t+1$, $t+2$, $t+3$, and $t+4$), scaled by the firm's stock price on the last day of the fiscal quarter ended prior to the conference call. We measure sales growth as the seasonal percentage change in quarterly sales ($SGROWTH_x$), measured one to four quarters after the conference call ($x = t+1$, $t+2$, $t+3$, and $t+4$).

We use analyst coverage, institutional ownership, and trading volume to capture capital market activities. To capture existing analyst coverage at the time of a given conference call, we measure the number of analysts (NAN_t) that issued an earnings estimate to I/B/E/S at any time between the previous conference call date and one day before the current conference call. We measure the next quarter's change in analyst coverage ($CNAN_{t+1}$) as the percentage change in *NAN* from quarter t to $t+1$. Thus, $CNAN_{t+1}$ requires one lag quarter of data and captures the change in total analyst coverage from the period before the conference call (roughly three months) to the period after the call. We

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15 In robustness checks, we use unscaled versions of *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* in the empirical analyses discussed in Section IV. In untabulated results, we find results that are similar to those presented in Tables 2 through 4. For the portfolio return tests in Table 5, we partition the sample into three (instead of four) groups because there is not enough variation in the unscaled versions of *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* to form quartiles in some quarters. Untabulated results indicate that our results hold using unscaled versions of the variables of interest.

16 It is interesting to point out that the coefficients on *COV_ANALYSTS_ABSENT* are smaller in magnitude than those on *NC_ANALYSTS* in all our regressions, a result consistent with the greater expected measurement error.

define institutional ownership as the number of institutional investors (NII_t) that own a firm's stock as of the most recent calendar quarter ended prior to the firm's conference call. We compute the next quarter's change in institutional investors ($CNII_{t+1}$) as the percentage change in *NII* from the calendar quarter ended prior to the conference call to the calendar quarter ended after the conference call. We define a firm's next quarter change in trading volume turnover ($CTURNOVER_{t+1}$) as the change in average daily turnover (volume divided by shares outstanding) from the 90 calendar days before to the 90 calendar days after the conference call, expressed in percentage terms.

We measure a firm's future stock return (*RET*) over the three-month period [m+1, m+3], where m is the month in which the conference call occurs. In robustness checks, we also measure two- and three-quarter-out stock returns, which are measured over the [m+4, m+6] and [m+7, m+9] windows, respectively.

We include several control variables in our analyses, including firm and stock characteristics measured prior to the conference call. When testing the association between analyst interest and future EPS and sales growth, we control for the current quarter's EPS and sales growth. We also control for firm size using the logarithm of market value of equity (*SIZE*), performance with income before extraordinary items divided by total assets (*ROA*), valuation using the book value of equity divided by market value of equity (*BTM*), and leverage with the book value of debt divided by the book value of equity (*LEVERAGE*). *PASTRET* is the size-adjusted return (raw return less return of the corresponding size decile) for the period [−91, −1], where day 0 is the date of the conference call. In the regressions of future capital market activities, we also control for stock volatility (*VOLATILITY*), defined as the standard deviation of daily size-adjusted returns for the period [−91, −1]. In the stock return regressions, we control for the most recent earnings surprise (*ENEWS*), *SIZE, BTM,* and the past 11-month return ($RET_{m-1,m-11}$) from the [m−11, m−1] period, where m is the month in which the conference call occurs. Finally, in the four-factor model, we use the $R_{Mt} - R_{ft}$, *SMB*, and *HML* factors defined in Fama and French (1996) and the momentum factor (*MOM*) defined in Carhart (1997). Appendix A summarizes all the variable definitions described above.

Table 1, Panel B shows descriptive statistics of the variables. The median values for ΔEPS_{t+1} and $SGROWTH_{t+1}$ are 0.00 and 0.10, respectively, indicating that firms are exhibiting higher top-line growth than bottom-line growth. The median firm is covered by six analysts (median $NAN =$ 6). The mean and median future percentage changes in the number of analysts ($CNAN_{t+1}$) are 0.05 and 0.00, respectively, and the interquartile range is from −0.06 to 0.13. These statistics indicate that for the median firm, analyst coverage is stable from quarter to quarter, but there is variation and a slightly right-skewed distribution in the change variable. The mean (median) $CNII_{t+1}$ is 0.02 (0.01), indicating that the number of institutional investors, on average, increases about 1 to 2 percent each quarter for our sample firms. $CTURNOVER_{t+1}$ has a mean value of 0.04 percent, indicating a small increase in daily trading volume each quarter, on average, for the sample firms.

Panels C and D in Table 1 show pair-wise correlations. As expected, *NC_ANALYSTS* is positively correlated with future changes in firm fundamentals (ΔEPS_{t+1} and $SGROWTH_{t+1}$) and capital market activities ($CNAN_{t+1}$, $CNII_{t+1}$, and $CTURNOVER_{t+1}$). Also as expected, *COV_ANALYSTS_ABSENT* is negatively correlated with $SGROWTH_{t+1}$, $CNAN_{t+1}$, $CNII_{t+1}$, and $CTURNOVER_{t+1}$. Furthermore, *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* are negatively correlated, as one would expect. In terms of the control variables, *NC_ANALYSTS* is negatively correlated with firm size (*SIZE*) and performance (*ROA*) and positively associated with firm leverage (*LEVERAGE*) and stock volatility (*VOLATILITY*), consistent with the notion that analysts tend to exert effort to learn about neglected firms that are difficult to understand.[17] Finally, only two

[17] Becoming an expert on neglected firms that are difficult to understand can increase an analyst's visibility among institutional investors and improve his or her career opportunities in the analyst labor market. We thank an anonymous reviewer for raising this point.

pairs of variables are highly correlated with each other with a correlation above |0.40|: *SIZE* and *VOLATILITY*, and *LEVERAGE* and *BTM*.[18]

IV. EMPIRICAL ANALYSES

Future Change in Firm Fundamentals

In the first part of our analyses, we examine whether *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* have predictive power for future changes in firm fundamentals. We estimate the following regression model (firm subscripts are suppressed for brevity):

$$\begin{aligned} CFUNDA_x = {} & \beta_0 + \beta_1 NC\_ANALYSTS_t + \beta_2 COV\_ANALYSTS\_ABSENT_t + \beta_3 CFUNDA_t \\ & + \beta_4 SIZE_t + \beta_5 ROA_t + \beta_6 BTM_t + \beta_7 LEVERAGE_t + \beta_8 PASTRET_t \\ & + Year\ Fixed\ Effects + \varepsilon, \end{aligned} \tag{1}$$

where $CFUNDA_x$ is either ΔEPS_x or $SGROWTH_x$ ($x = t+1$, $t+2$, $t+3$, or $t+4$). Prediction 1 states that the estimated coefficients on $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ should be positive and negative, respectively.

We control for the current-quarter change in firm fundamental $CFUNDA_t$ (= ΔEPS_t or $SGROWTH_t$). The estimated coefficient on $CFUNDA_t$ depends on the time-series property of $CFUNDA_t$. For ΔEPS, we expect it to be serially positively correlated in the first three lags and negatively correlated in the fourth lag (Bernard and Thomas 1989). For *SGROWTH*, we expect it to follow an autoregressive process, with a nonnegative coefficient decreasing with x. We include other contemporaneous firm characteristics to control for cross-sectional differences that may explain the variations in $CFUNDA_x$, although the relations between such factors and EPS growth and sales growth may differ. Firm size (*SIZE*) captures a firm's market power and competitive position, which should be positively associated with earnings power (ΔEPS_t). However, smaller firms generally have higher sales growth potential than larger firms and, hence, we expect *SIZE* to be negatively associated with $SGROWTH_t$. Operating performance (*ROA*) is expected to have negative coefficients, as firms with high existing levels of earnings are less likely to have higher EPS growth and sales growth. Book-to-market (*BTM*) is a proxy for a firm's investment opportunity set. Since firms with low *BTM* exhibit higher growth, we expect a negative coefficient on *BTM*. Leverage (*LEVERAGE*) captures the capital structure of the firm and, all else being equal, firms with higher leverage should exhibit higher earnings growth, but the expected effect of $LEVERAGE_t$ on sales growth is less clear. We include past stock return (*PASTRET*) to control for confounding events, such as industry-specific news or managerial voluntary disclosures, that may drive both analyst interest and future firm fundamentals, and we expect $PASTRET_t$ to have a positive coefficient.[19]

Table 2, Panel A shows the results of the ΔEPS_x regressions. We cluster standard errors by firm (Rogers 1993). Columns (1) through (4) show that the estimated coefficients on *NC_ANALYSTS* are significantly positive when ΔEPS is measured for the next four quarters. The magnitude of the coefficient (0.009) in column (1) suggests that an interquartile shift in the value of *NC_ANALYSTS* translates into a 0.34 percent (= 0.009 × 0.38) change in ΔEPS, which represents 17.1 percent (= 0.0034/0.02) of the interquartile range in one-quarter-ahead ΔEPS. These results are consistent with analyst interest having predictive power for ΔEPS in the next four quarters. In contrast, *COV_*

[18] In subsequent regression analysis, we conduct multicollinearity diagnostics whenever explanatory variables have correlations above |0.4|. In each case, we find that the variance inflation factors are below 2 for the variables tested, suggesting that multicollinearity is not an issue.

[19] We thank an anonymous reviewer for pointing out this possibility and offering this solution.

TABLE 2

Regressions of Future Growth on the Number of Non-Covering Analysts and Absent Covering Analysts on a Conference Call

Panel A: Regressions of Future Earnings Per Share Growth (ΔEPS_x)

Dependent Variable	Pred. Sign	ΔEPS_{t+1} (1)	ΔEPS_{t+2} (2)	ΔEPS_{t+3} (3)	ΔEPS_{t+4} (4)
$NC\_ANALYSTS_t$	+	0.009***	0.008***	0.005*	0.005*
		(3.88)	(3.26)	(1.84)	(1.83)
$COV\_ANALYSTS\_ABSENT_t$	−	0.002	0.001	−0.001	0.002
		(1.27)	(0.55)	(−0.62)	(0.95)
ΔEPS_t	+/−	0.173***	0.088***	0.009	−0.411***
		(7.26)	(4.76)	(0.45)	(−15.37)
$SIZE_t$	+	0.000	0.000	0.000	0.003***
		(−0.69)	(−0.25)	(−0.23)	(5.70)
ROA_t	−	−0.095***	−0.125***	−0.115***	−0.628***
		(−4.39)	(−5.41)	(−5.27)	(−11.13)
BTM_t	−	−0.041***	−0.023***	−0.012**	−0.017***
		(−7.93)	(−4.48)	(−2.56)	(−3.59)
$LEVERAGE_t$	+	0.000	0.001	0.001	0.001
		(0.18)	(0.36)	(0.63)	(0.52)
$PASTRET_t$	+	0.061***	0.054***	0.020***	0.009*
		(13.06)	(10.69)	(3.80)	(1.95)
Intercept		0.024***	0.014*	0.010*	−0.008
		(3.03)	(1.92)	(1.66)	(−1.22)
Year Fixed Effects		Yes	Yes	Yes	Yes
n		49,378	49,373	49,295	49,191
Adj. R^2		0.072	0.035	0.018	0.207

Panel B: Regressions of Future Sales Growth ($SGROWTH_x$)

Dependent Variable	Pred. Sign	$SGROWTH_{t+1}$ (1)	$SGROWTH_{t+2}$ (2)	$SGROWTH_{t+3}$ (3)	$SGROWTH_{t+4}$ (4)
$NC\_ANALYSTS_t$	+	0.017*	0.038***	0.028**	0.031**
		(1.67)	(3.09)	(2.13)	(1.98)
$COV\_ANALYSTS\_ABSENT_t$	−	−0.006	0.000	0.001	−0.001
		(−1.30)	(0.07)	(0.16)	(−0.35)
$SGROWTH_t$	+	0.563***	0.356***	0.218***	0.019
		(24.34)	(16.06)	(11.55)	(1.04)
$SIZE_t$	−	−0.004***	−0.007***	−0.009***	−0.011***
		(−2.93)	(−3.61)	(−4.29)	(−4.57)
ROA_t	−	−0.432***	−0.581***	−0.779***	−1.297***
		(−4.72)	(−4.76)	(−5.32)	(−7.31)
BTM_t	−	−0.094***	−0.119***	−0.131***	−0.140***
		(−12.17)	(−13.25)	(−13.27)	(−11.55)
$LEVERAGE_t$		0.003	−0.002	−0.006***	−0.009***
		(1.61)	(−0.90)	(−2.76)	(−3.47)
$PASTRET_t$	+	0.133***	0.211***	0.247***	0.180***
		(6.66)	(10.44)	(12.70)	(8.54)

(continued on next page)

TABLE 2 (continued)

Dependent Variable	Pred. Sign	$SGROWTH_{t+1}$ (1)	$SGROWTH_{t+2}$ (2)	$SGROWTH_{t+3}$ (3)	$SGROWTH_{t+4}$ (4)
Intercept		0.143***	0.187***	0.226***	0.278***
		(8.82)	(10.13)	(10.90)	(11.71)
Year Fixed Effects		Yes	Yes	Yes	Yes
n		46,937	46,902	46,817	46,728
Adj. R^2		0.369	0.172	0.087	0.034

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a two-tailed test and standard errors clustered by firm. The t-statistic is presented in parentheses under the estimated coefficient.
The dependent variable is future growth in earnings per share (ΔEPS_{t+x}) in Panel A, and future sales growth ($SGROWTH_{t+x}$) in Panel B.
Variables not shown below are defined in Appendix A.

Variable Definitions:
ΔEPS_{t+x} = seasonal difference in earnings per share (diluted) excluding extraordinary items scaled by the firm's stock price as of the fiscal quarter ended prior to the conference call (quarter t), and $x = 1,2,3,4$ are the four fiscal quarters ended after the conference call;
$SGROWTH_{t+x}$ = seasonal percentage change in quarterly sales;
NC_ANALYSTS = number of analysts that asked a question on the firm's conference call, but did not cover the firm as of the date of the conference call, scaled by the number of analysts that asked a question on the firm's conference call (*NUMCALLERS*); and
COV_ANALYSTS_ABSENT = number of analysts that covered the firm as of the date of the conference call, did not ask a question on the conference call, but did ask a question on the previous conference call, scaled by *NUMCALLERS*.

ANALYSTS_ABSENT does not exhibit a significant explanatory power for future ΔEPS. Regarding the control variables, ROA_t, BTM_t, and $PASTRET_t$ exhibit significant associations in the predicted directions. Finally, consistent with the results from Bernard and Thomas (1989), ΔEPS_t exhibits a positive estimated coefficient in the ΔEPS_{t+1} and ΔEPS_{t+2} regressions and a negative coefficient in the ΔEPS_{t+4} regression.

Table 2, Panel B shows the results of the $SGROWTH_x$ regressions. Columns (1) through (4) indicate that *NC_ANALYSTS* is positively associated with sales growth one to four quarters later, with the coefficient highest for two quarters out ($SGROWTH_{t+2}$). In terms of economic significance, the coefficient of 0.038 on *NC_ANALYSTS* in the $SGROWTH_{t+2}$ regression (column (2)) suggests that an interquartile shift in the value of *NC_ANALYSTS* translates into a 1.44 percent (= 0.038 × 0.38) change in sales growth two quarters later, which represents 6.00 percent (= 0.0144/0.24) of the interquartile range in future sales growth. These findings suggest that firms with increased analyst interest are associated with sales growth over the next one to four quarters. Similar to the results in Panel A, *COV_ANALYSTS_ABSENT* exhibits no association with future sales growth. The control variables again generally exhibit significant associations in the predicted directions.

Overall, we find that *NC_ANALYSTS* is positively related to future EPS growth and sales growth, consistent with our first prediction, whereas *COV_ANALYSTS_ABSENT* exhibits no predictive power with respect to changes in future fundamentals. As discussed earlier, greater measurement error in *COV_ANALYSTS_ABSENT* (relative to *NC_ANALYSTS*) likely attenuates the coefficients more toward zero, resulting in less power in our tests of analyst disinterest.

Future Change in Capital Market Activities

We next examine whether a change in analyst interest has predictive power for future changes in capital market activities, such as analyst coverage, institutional ownership, and trading volume, using the following regression model (firm subscripts are suppressed):

$$CMACTIVITY_{t+1} = \beta_0 + \beta_1 NC\_ANALYSTS_t + \beta_2 COV\_ANALYSTS\_ABSENT_t + \beta_3 CMACTIVITY_t + \beta_4 SIZE_t + \beta_5 ROA_t + \beta_6 BTM_t + \beta_7 LEVERAGE_t + \beta_8 PASTRET_t + \beta_9 VOLATILITY_t + Year\ Fixed\ Effects + \varepsilon, \quad (2)$$

where $CMACTIVITY_{t+1}$ takes one of the following three variables: $CNAN_{t+1}$, $CNII_{t+1}$, or $CTURNOVER_{t+1}$, as defined in Section III and summarized in Appendix A. Prediction 2 states that the estimated coefficients on *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* should be positive and negative, respectively.

We include the contemporaneous change in capital market activity, $CMACTIVITY_t$ (= $CNAN_t$, $CNII_t$, or $CTURNOVER_t$) to address any serial correlation issues. We control for other firm characteristics that may explain cross-sectional variation in $CMACTIVITY_{t+1}$. In particular, prior studies find that firm size (*SIZE*) is positively related to existing levels of analyst coverage and institutional ownership (e.g., O'Brien and Bhushan 1990). As such, we expect future changes in coverage and ownership to be smaller for larger firms. Operating performance (*ROA*) attracts the interest of analysts and institutional investors and, thus, is expected to be positively related to changes in the next quarter's capital market activities. Book-to-market (*BTM*) proxies for the investment opportunity set, and it is expected to be negatively associated with changes in capital market activities. Leverage (*LEVERAGE*) captures the capital structure of the firm. As in Equation (1), we include past stock return (*PASTRET*) to control for confounding events that have occurred since the prior conference call date, and expect the estimated coefficient to be positive. We include one additional control variable, past stock volatility (*VOLATILITY*), which is expected to have a negative association with future changes in capital market activities because volatile stocks are less attractive to investors.

Table 3 summarizes the estimation of Equation (2). Column (1) reports the results for the $CNAN_{t+1}$ regression. Consistent with our expectation, the coefficient of 0.085 on *NC_ANALYSTS* is significant at the 1 percent level (t-stat = 11.06), and its magnitude suggests that an interquartile shift in the value of *NC_ANALYSTS* translates into a 3.23 percent (= 0.085 × 0.38) change in analyst coverage. Since the interquartile range of $CNAN_{t+1}$ is 0.19 (Table 1, Panel B), the marginal effect of $NC\_ANALYSTS_t$ represents 17.0 percent (= 0.032/0.190) of that range. The coefficient on *COV_ANALYSTS_ABSENT* is −0.046 (t-stat = −15.34), indicating that an interquartile shift in *COV_ANALYSTS_ABSENT* translates into a 1.84 percent (= −0.046 × 0.40) reduction in the number of analysts following, or 9.68 percent (= 0.0184/0.190) of the interquartile range in $CNAN_{t+1}$.

When interpreting our results, we note two benchmarks. First, the magnitudes of the incremental changes we document are comparable to prior work, which typically finds a mean change in analyst coverage of less than one analyst after a disclosure event (e.g., Francis, Hanna, and Philbrick 1997; Healy, Hutton, and Palepu 1999; Irani and Karamanou 2003; Bushee, Jung, and Miller 2011). Second, the potential for increases in analyst coverage among our sample firms is not large, on average, because the mean number of analysts is 7.9 and the median is 6.0 (Table 1, Panel B) and because analyst coverage is very stable over time. Thus, one should expect an *unconditional* increase of less than one analyst per quarter in the first place. Untabulated analysis shows that about 20 percent of non-covering analysts initiate coverage within one year of showing up on a firm's earnings conference call for the first time.[20] Conversely, about 14 percent of covering analysts who are absent from a given call eventually drop coverage within one year.

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[20] We view such a "conversion rate" to be significant for several reasons: (1) generally, many analysts dial into many firms' conference calls because the cost is relatively low (just the time required); (2) not every analyst participating on the conference call will ultimately initiate coverage for various reasons (Soltes 2014), and when analysts do decide to initiate coverage, it may take more than a year; and (3) analyst job changes and reassignments introduce measurement errors that reduce the conversion rate.

TABLE 3

Regressions of Future Changes in Analyst Coverage and Institutional Ownership

Dependent Variable	Pred. Sign	$CNAN_{t+1}$ (1)	$CNII_{t+1}$ (2)	$CTURNOVER_{t+1}$ (3)
$NC\_ANALYSTS_t$	+	0.085***	0.008***	0.022**
		(11.06)	(3.40)	(2.13)
$COV\_ANALYSTS\_ABSENT_t$	−	−0.046***	−0.004***	−0.003
		(−15.34)	(−4.87)	(−0.53)
$CNAN_t$		−0.121***		
		(−16.40)		
$CNII_t$			0.000	
			(0.00)	
$CTURNOVER_t$				−0.097***
				(−5.01)
$SIZE_t$	−	−0.007***	−0.006***	−0.016***
		(−7.95)	(−14.77)	(−4.34)
ROA_t	+	0.161***	0.086***	−0.122*
		(5.76)	(4.88)	(−1.71)
BTM_t	−	−0.059***	−0.018***	−0.001
		(−14.86)	(−11.18)	(−0.08)
$LEVERAGE_t$		0.002*	0.001	0.019***
		(1.92)	(1.37)	(5.18)
$PASTRET_t$	+	0.090***	0.241***	0.224***
		(10.63)	(57.99)	(7.77)
$VOLATILITY_t$	−	−0.381***	−0.224***	−6.892***
		(−3.10)	(−3.74)	(−8.39)
Intercept		0.144***	0.051***	0.337***
		(10.69)	(10.13)	(6.28)
Year Fixed Effects		Yes	Yes	Yes
n		46,983	47,522	49,525
Adj. R^2		0.041	0.153	0.041

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a two-tailed test and standard errors clustered by firm. The t-statistic is presented in parentheses under the estimated coefficient.
Table 3 presents results of regressions of next quarter's change in analyst coverage ($CNAN_{t+1}$), institutional ownership ($CNII_{t+1}$), and trading volume turnover ($CTURNOVER_{t+1}$) on the number of non-covering analysts ($NC\_ANALYSTS_t$) and covering analysts who are absent ($COV\_ANALYSTS\_ABSENT_t$) from a firm's earnings conference call, as well as on control variables.
Variables not shown below are defined in Appendix A.

Variable Definitions:
$CNAN_{t+1}$ = percentage change in the number of analysts that covered the firm from the quarter before the conference call to the quarter after the conference call;
$CNII_{t+1}$ = percentage change in the number of institutional investors that owned the firm's stock from the calendar quarter ended prior to the conference call to the calendar quarter ended after the conference call;
$CTURNOVER_{t+1}$ = change in average daily turnover (volume divided by shares outstanding) from the 90 days before to the 90 days after the conference call, expressed in percentage terms;
NC_ANALYSTS = number of analysts that asked a question on the firm's conference call, but did not cover the firm as of the date of the conference call, scaled by the number of analysts that asked a question on the firm's conference call (*NUMCALLERS*); and
COV_ANALYSTS_ABSENT = number of analysts that covered the firm as of the date of the conference call, did not ask a question on the conference call, but did ask a question on the previous conference call, scaled by *NUMCALLERS*.

Regarding the control variables, Table 3, column (1) shows that all the control variables significantly explain changes in analyst coverage over the next quarter. In particular, firms that have already experienced greater increases in analyst coverage, are larger in size, and have high book-to-market and volatility exhibit a decrease (or a smaller increase) in future analyst coverage. Moreover, firms with high operating performance, leverage, and past returns exhibit higher increases in analyst coverage over the next quarter. Thus, the aforementioned results for *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* are incremental to observable firm fundamentals and stock characteristics.

In Table 3, column (2) we report the regression results for the percentage change in the number of institutional investors, $CNII_{t+1}$. We find a positive coefficient of 0.008 (t-stat = 3.40) on *NC_ANALYSTS,* significant at the 1 percent level. Moving *NC_ANALYSTS* from the first to the third quartile translates into a 0.3 percent (= 0.008 × 0.38) increase in the percentage of institutional ownership, or 2.53 percent (= 0.003/0.12) of the interquartile range in $CNII_{t+1}$. The coefficient on *COV_ANALYSTS_ABSENT* is −0.004 (t-stat = −4.87). Moving *COV_ANALYSTS_ABSENT* from the first to the third quartile translates into a 0.16 percent (= −0.001 × 0.40) decrease in $CNII_{t+1}$, or 1.33 percent (= 0.0016/0.12) of the interquartile range in $CNII_{t+1}$. We benchmark our results against prior studies that have documented increases of less than 1 percent in institutional ownership per quarter following changes in firms' information environment (Bushee and Noe 2000; Covrig, DeFond, and Hung 2007; Bushee et al. 2011). For example, Lehavy and Sloan (2008) show that the unconditional average quarterly percentage change in institutional ownership for firms is nearly zero from 1982 to 2004, with a mean of 0.10 percent and a median of 0.00. Only in the highest two deciles of firms ranked by changes in institutional ownership is the average greater than 0.29 percent. Therefore, we believe that the economic significance of *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* in explaining $CNII_{t+1}$ is relatively large. The coefficients on the control variables are largely expected and similar to those reported under column (1), except that *LEVERAGE* exhibits no association with future increases in institutional ownership.

Finally, we examine whether changes in analyst interest are related to future changes in trading volume turnover. In Table 3, column (3) the coefficient on *NC_ANALYSTS* is 0.022 and significant (t-stat = 2.13), but the coefficient on *COV_ANALYSTS_ABSENT* is insignificant (t-stat = −0.53). A shift in the value of *NC_ANALYSTS* from the first to the third quartile translates into a 0.84 percent (= 0.022 × 0.38) increase in $CTURNOVER_{t+1}$, or 2.88 percent (= 0.008/0.29) of the interquartile range in $CTURNOVER_{t+1}$. All the control variables exhibit a significant association with the dependent variable and, with one exception (*BTM*), in the predicted direction.

Overall, consistent with our Prediction 2, we find that changes in analyst interest predict subsequent capital market activities. *NC_ANALYSTS* is positively related to future changes in analyst coverage, institutional ownership, and trading volume, whereas *COV_ANALYSTS_ABSENT* is negatively related to future changes in analyst coverage and institutional ownership.

Future Stock Returns

Finally, we examine whether changes in analyst interest predict future stock returns. We first use regression analyses to test the predictive power of $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ for future stock returns. For each quarterly conference call, we calculate the three-month stock return (*RET*) from month m+1 to m+3, where m is the month during which the conference call occurs. We control for earnings news and common return factors in the following regression model:

$$RET = \beta_0 + \beta_1 NC\_ANALYSTS_t + \beta_2 COV\_ANALYSTS\_ABSENT_t + \beta_3 ENEWS_t + \beta_4 SIZE_t + \beta_5 BTM_t + \beta_6 RET_{m-1,m-11} + \varepsilon. \quad (3)$$

TABLE 4

Regressions of Future Stock Returns on Analyst Interest Variables

Dependent Variable	Pred. Sign	*RET* (1)	*RET* (2)
Intercept		0.051	0.050
		(1.20)	(1.20)
$NC\_ANALYSTS_t$	+	0.023***	0.023***
		(2.76)	(2.72)
$COV\_ANALYSTS\_ABSENT_t$	–	−0.005**	−0.005**
		(−2.06)	(−2.18)
$ENEWS_t$	+	3.25***	3.27***
		(8.60)	(8.69)
$SIZE_t$		−0.006	−0.006
		(−1.64)	(−1.62)
BTM_t		0.010	0.009
		(0.99)	(0.89)
$RET_{m-11,m-1}$		−0.019	−0.019
		(−1.30)	(−1.34)
$CNAN_t$			−0.012*
			(−1.80)
$CNII_t$			0.014
			(0.79)
$CTURNOVER_t$			0.007
			(0.22)
Adj. R^2		0.054	0.057

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a one-tailed test. The Fama and MacBeth (1973) t-statistic is presented in parentheses under the estimated coefficient.
Table 4 presents results of regressions of future stock returns (*RET*) on the number of non-covering analysts ($NC\_ANALYSTS_t$) and covering analysts who are absent ($COV\_ANALYSTS\_ABSENT_t$) from a firm's earnings conference call, as well as on control variables. The coefficient estimates are the average of quarterly estimates over 27 quarters from the third quarter of 2002 to the first quarter of 2009.
Variables not shown below are defined in Appendix A.

Variable Definitions:
RET = measured over a three-month window [m+1, m+3], where m is the month in which a firm's conference call occurred;
NC_ANALYSTS = number of analysts that asked a question on the firm's conference call, but did not cover the firm as of the date of the conference call, scaled by the number of analysts that asked a question on the firm's conference call (*NUMCALLERS*); and
COV_ANALYSTS_ABSENT = number of analysts that covered the firm as of the date of the conference call, did not ask a question on the conference call, but did ask a question on the previous conference call, scaled by *NUMCALLERS*.

All variables are as previously defined. Prediction 3 states that the estimated coefficients on *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* should be positive and negative, respectively.

Table 4 reports the regression results. Column (1) shows that $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ are positively and negatively, respectively, associated with future stock returns. In column (2) we further control for contemporaneous changes in analyst coverage ($CNAN_t$), institutional ownership ($CNII_t$), and share turnover ($CTURNOVER_t$) under the premise that contemporaneous changes in capital market activities also predict future stock returns and that $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ are correlated with these variables. Column (2) indicates that both the magnitude and significance of the estimated coefficients on $NC\_ANALYSTS_t$

and $COV\_ANALYSTS\_ABSENT_t$ are not affected by the inclusion of the additional controls. Regarding control variables, *ENEWS* has a statistically positive coefficient, consistent with the positive link between fundamental news and stock returns. The coefficient on *SIZE* is negative and marginally significant, while the coefficients on *BTM* and $RET_{m-1,m-11}$ are statistically insignificant. In sum, these results confirm our conjecture that analyst interest is an early indicator of future stock returns.

Next, we gauge the economic significance of the return results by comparing subsequent three-month stock returns (*RET*) between the top and bottom quartiles of the analyst interest variables. The findings are presented in Table 5, Panel A. *RET* increases monotonically from 1.69 percent in the bottom *NC_ANALYSTS* quartile to 3.56 percent in the top quartile. A return difference of 1.87 percent (t-stat = 3.66) between the top and bottom quartiles is both statistically and economically significant. When partitioning on *COV_ANALYSTS_ABSENT*, we find that *RET* decreases from 3.15 percent in the bottom quartile to 1.48 percent in the top quartile. A return difference of −1.67 percent (t-stat = −2.81) between the top and bottom quartiles is also significant.

Finally, we use a four-factor model to show that the aforementioned return differences are not attributable to common return factors. Since risk factors are available monthly, we match $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ with stock returns in months m+1, m+2, and m+3, where m is the month of the conference call. For each month, we independently sort the sample into quartiles based on $NC\_ANALYSTS_t$ or $COV\_ANALYSTS\_ABSENT_t$, resulting in four $NC\_ANALYSTS_t$ and four $COV\_ANALYSTS\_ABSENT_t$ portfolios. We calculate portfolio returns, R_{it}, as the average stock returns of firms in each portfolio, and we estimate abnormal returns using the following four-factor model for each portfolio:

$$R_{it} - R_{ft} = a + b_{iM}(R_{Mt} - R_{ft}) + s_i SMB_t + h_i HML_t + m_i MOM_t + \varepsilon_{it}, \tag{4}$$

where $R_{Mt} - R_{ft}$, SMB_t, and HML_t are defined in Fama and French (1996), and MOM_t is the momentum factor defined in Carhart (1997). The four-factor data are from Kenneth French's website (see: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/). The intercept, a, provides an estimate of the monthly abnormal returns earned by each $NC\_ANALYSTS_t$ or $COV\_ANALYSTS\_ABSENT_t$ portfolio, after controlling for these four factors.

Table 5, Panel B summarizes the estimation of Equation (4) for all the portfolios. The intercepts from the four-factor model increase monotonically with $NC\_ANALYSTS_t$ from 0.41 percent for portfolio Q1 to 0.516 percent for portfolio Q4. A hedge portfolio with a long position in Q4 stocks and a short position in Q1 stocks yields a significant abnormal return of 0.475 percent per month or 5.7 percent per year. For the $COV\_ANALYSTS\_ABSENT_t$ portfolios, abnormal monthly returns decrease from 0.219 percent in portfolio Q1 to −0.007 percent in portfolio Q4. A hedge portfolio yields a significant abnormal return of −0.226 percent per month or −2.7 percent per year.

Overall, both regression and portfolio analyses suggest that the analyst interest variables predict future stock returns and that the effects are both economically and statistically significant, consistent with Prediction 3. *NC_ANALYSTS* is positively related to subsequent stock returns, whereas *COV_ANALYSTS_ABSENT* is negatively related to subsequent stock returns.

Testing the Investor Recognition Story after Controlling for Future Fundamentals

As discussed in Section II, the theories of both McNichols and O'Brien (1997) and Merton (1987) suggest that changes in analyst interest predict subsequent capital market activities and future stock returns. Although these two theories are not mutually exclusive, we attempt to shed light on whether Merton's (1987) investor recognition story holds after controlling for future fundamentals. We again note that Merton's (1987) theory has no direct implications for changes in future fundamentals.

TABLE 5

Portfolio Analysis Based on Analyst Interest Variables

Panel A: Three-Month Returns across Four Quartiles Based on Analyst Interest Variables

Sorted by	Q1	Q2	Q3	Q4	Q4 − Q1
$NC\_ANALYSTS_t$	1.69%	1.71%	2.12%	3.56%	1.87%***
					(3.66)
$COV\_ANALYSTS\_ABSENT_t$	3.15%	1.44%	1.72%	1.48%	−1.67%***
					(−2.81)

Panel B: The Four-Factor Model on Monthly Portfolio Returns Based on Analyst Interest Variables

	Intercept	$R_M - R_f$	*SMB*	*HML*	*MOM*	Adj. R^2
Sorted by $NC\_ANALYSTS_t$						
Q1	0.041	1.042	0.798	0.004	−0.222	0.987
	(0.47)	(43.56)	(19.46)	(0.10)	(−13.68)	
Q2	0.127	1.042	0.637	−0.253	−0.182	0.964
	(0.95)	(28.01)	(9.99)	(−4.33)	(−7.20)	
Q3	0.135	1.100	0.850	0.043	−0.192	0.981
	(1.23)	(35.91)	(16.17)	(0.90)	(−9.23)	
Q4	0.516***	1.098	0.886	0.027	−0.217	0.966
	(3.48)	(26.48)	(12.46)	(0.41)	(−7.72)	
Q4 − Q1	0.475***	0.056	0.088	0.023	0.005	0.069
	(2.78)	(1.30)	(1.65)	(0.34)	(0.07)	
Sorted by $COV\_ANALYSTS\_ABSENT_t$						
Q1	0.219**	1.070	0.910	0.061	−0.175	0.981
	(2.03)	(35.48)	(17.59)	(1.29)	(−8.51)	
Q2	−0.048	1.060	0.738	−0.057	−0.175	0.977
	(−0.43)	(34.26)	(13.90)	(−1.17)	(−8.30)	
Q3	0.043	1.055	0.750	−0.019	−0.240	0.981
	(0.42)	(36.40)	(15.08)	(−0.41)	(−12.17)	
Q4	−0.007	1.082	0.759	0.001	−0.263	0.982
	(−0.09)	(36.43)	(14.90)	(0.02)	(−13.03)	
Q4 − Q1	−0.226**	0.012	−0.151	−0.060	−0.088	0.286
	(−2.24)	(0.12)	(−2.79)	(−1.45)	(−4.98)	

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a one-tailed test. The White heteroscedasticity-adjusted t-statistic is presented in parentheses under the coefficient estimate.

Table 5 presents monthly portfolio returns based on sorting observations into quartiles based on $NC\_ANALYSTS_t$ or $COV\_ANALYSTS\_ABSENT_t$ (both defined in Appendix A). Panel A reports raw returns across quartiles, as well as the Q4 − Q1 hedge portfolio. Panel B reports the results of the four-factor model shown in Equation (4). The Intercept represents the monthly excess return for each portfolio, after controlling for four factors (obtained from Kenneth French's website at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/). As the factor data are monthly, we match $NC\_ANALYSTS_t$ or $COV\_ANALYSTS\_ABSENT_t$ from the conference month *m* with monthly returns from *m*+1 to *m*+3. The sample period in Panel B includes 84 months, from July 2002 to June 2009.

Table 6 reports regression results for future capital market activities and future stock returns after controlling for future fundamentals. In particular, we include future EPS and sales growth (ΔEPS_{t+1} and $SGROWTH_{t+1}$) as additional variables into regression Equations (2) and (3). Panel A shows that the coefficients on *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* are similar to those

TABLE 6

Testing the Firm Fundamentals and Investor Visibility Stories

Panel A: Regression of Future Capital Market Activities after Controlling for Future Firm Fundamentals

Dependent Variable	Pred. Sign	$CNAN_{t+1}$ (1)	$CNII_{t+1}$ (2)	$CTURNOVER_{t+1}$ (3)
$NC\_ANALYSTS_t$	+	0.088***	0.010***	0.023**
		(10.87)	(4.05)	(2.16)
$COV\_ANALYSTS\_ABSENT_t$	−	−0.046***	−0.005***	−0.006
		(−14.52)	(−5.24)	(−1.00)
$CNAN_t$		−0.125***	0.014***	0.014
		(−16.39)	(6.43)	(1.00)
$CNII_t$		0.159***	0.036***	0.215***
		(12.43)	(4.19)	(6.48)
$CTURNOVER_t$		0.002	0.003**	−0.118***
		(1.01)	(2.45)	(−4.70)
$SIZE_t$	−	−0.006***	−0.005***	0.000
		(−6.37)	(−11.91)	(0.13)
ROA_t	+	0.176***	0.114***	0.113
		(6.62)	(7.70)	(1.50)
BTM_t	−	−0.056***	−0.023***	−0.023**
		(−13.09)	(−13.30)	(−2.07)
$LEVERAGE_t$		0.001	0.000	0.016***
		(0.74)	(0.22)	(3.96)
$PASTRET_t$	+	0.095***	0.270***	0.159***
		(7.25)	(42.12)	(2.94)
$VOLATILITY_t$	−	−0.288***	−0.393***	−2.834***
		(−2.71)	(−7.22)	(−5.43)
ΔEPS_{t+1}		−0.022*	0.024***	−0.213***
		(−1.91)	(4.09)	(−3.89)
$SGROWTH_{t+1}$		0.019***	0.016***	0.009
		(6.29)	(7.71)	(0.57)
Intercept		0.128***	0.047***	0.111***
		(9.21)	(8.65)	(2.63)
Year Fixed Effects		Yes	Yes	Yes
n		42,900	43,234	43,266
Adj. R^2		0.047	0.131	0.029

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a one-tailed test.
The t-statistic is presented in parentheses under the estimated coefficient.

(continued on next page)

reported in Table 3. Likewise, the results presented in Panel B indicate that the predictive powers of *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* for subsequent stock returns remain intact after controlling for ΔEPS_{t+1} and $SGROWTH_{t+1}$. Collectively, these results suggest that Merton's (1987) investor recognition story plays a role in the link between analyst interest and future capital market activities and stock returns.

TABLE 6 (continued)

Panel B: Regression of Future Stock Returns after Controlling for Future Firm Fundamentals

Dependent Variable	Pred. Sign	*RET* (1)	*RET* (2)
Intercept		0.023	0.023
		(0.56)	(0.56)
$NC\_ANALYSTS_t$	+	0.022***	0.022***
		(2.67)	(2.78)
$COV\_ANALYSTS\_ABSENT_t$	–	–0.006***	–0.006***
		(–2.99)	(–2.89)
ΔEPS_{t+1}	+	0.996***	0.997***
		(7.08)	(6.86)
$SGROWTH_{t+1}$	+	0.057***	0.056***
		(4.84)	(4.99)
$ENEWS_t$	+	3.04***	3.01***
		(6.27)	(6.24)
$SIZE_t$		–0.004	–0.004
		(–1.08)	(–1.07)
BTM_t		0.019**	0.017*
		(2.10)	(1.76)
$RET_{m-11,m-1}$		–0.038**	–0.034**
		(–2.49)	(–2.34)
$CNAN_t$			–0.013**
			(–2.16)
$CNII_t$			–0.014
			(–0.51)
$CTURNOVER_t$			–0.013
			(–0.32)
Adj. R^2		0.088	0.093

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a one-tailed test. The Fama and MacBeth (1973) t-statistic is presented in parentheses under the estimated coefficient.
Panel A presents results of regressions of next quarter's change in analyst coverage ($CNAN_{t+1}$), institutional ownership ($CNII_{t+1}$), and trading volume turnover ($CTURNOVER_{t+1}$) on the number of non-covering analysts ($NC\_ANALYSTS_t$) and covering analysts who are absent ($COV\_ANALYSTS\_ABSENT_t$) from a firm's earnings conference call, as well as on control variables. The results are comparable to those presented in Table 3, except that future earnings growth (ΔEPS_{t+1}) and sales growth ($SGROWTH_{t+1}$) are included in the regression to control for changes in future fundamentals. Panel B presents results of regressions of future stock returns (*RET*) on $NC\_ANALYSTS_t$, $COV\_ANALYSTS\_ABSENT_t$, and control variables. The results are comparable to those presented in Table 4, except that future earnings growth (ΔEPS_{t+1}) and sales growth ($SGROWTH_{t+1}$) are included in the regression to control for changes in future fundamentals.
All variables are defined in Appendix A.

V. ALTERNATIVE EXPLANATIONS AND ROBUSTNESS TESTS

Alternative Explanation: Confounding Information

Confounding information, such as industry news or information released to the market by a given firm, may cause analysts to become interested in that firm. For example, if the first reporting firm in an industry beats earnings expectations substantially due to an industry shock, then analysts will flock to the next firm's call to understand how the shock will manifest. In this case, news from

another firm may be driving analyst interest, future firm fundamentals, and stock returns.[21] To address this concern, we have included *PASTRET* as a control variable in Tables 2 and 3, where *PASTRET* is stock return over the past three months up to the conference call date. In the return tests reported in Table 4, we follow the literature and use the standard 11-month stock returns, $RET_{m-11,m-1}$, to proxy for price momentum. To check the sensitivity of the return results, we add size-adjusted *PASTRET* as an additional control variable. Results (not tabulated) are qualitatively similar to those presented in Table 4. Specifically, the t-statistic of the coefficient on *NC_ANALYSTS* is slightly stronger while the t-statistic of the coefficient on *COV_ANALYSTS_ABSENT* is slightly weaker, relative to the models without *PASTRET*.

Alternative Explanation: Upward Trend in Conference Call Likelihood and Coverage

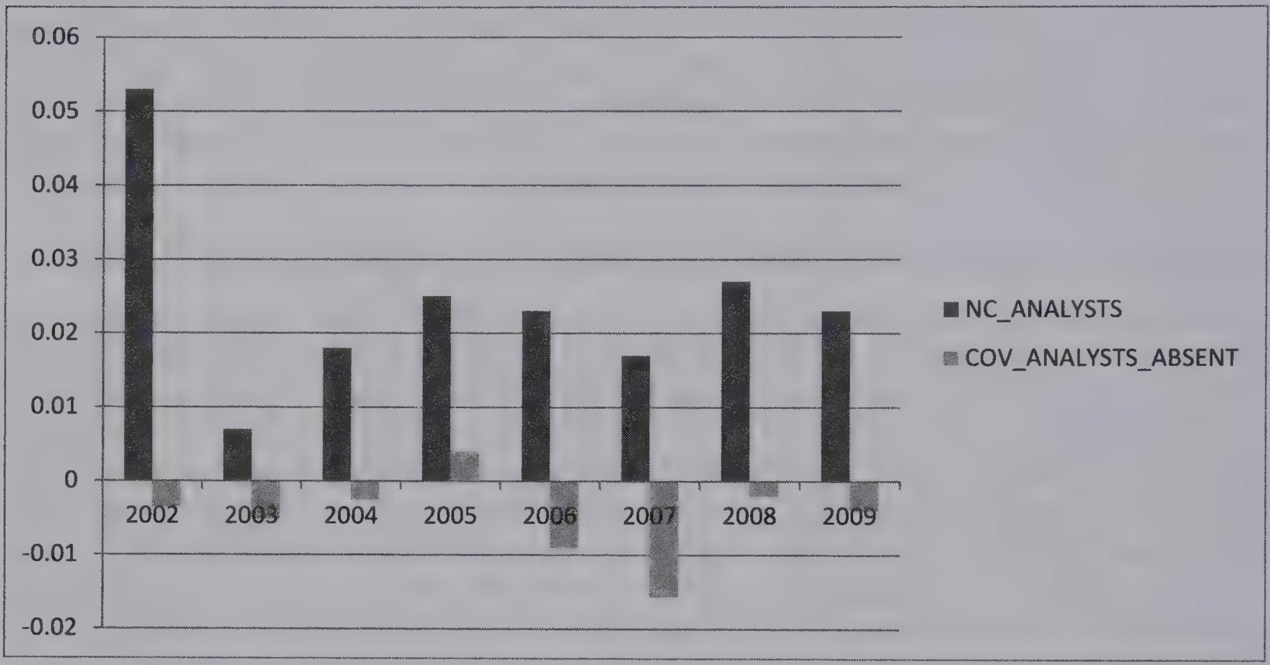
Another alternative explanation is that there is an upward trend in conference call data (see Table 1, Panel A) and our results could be driven by observations from only a few years. We investigate this possibility using a relatively stable subsample. In particular, we require each firm to have data for at least 21 quarters out of our 27-quarter sample period (more than 75 percent of the quarters). The resultant subsample has 35,554 firm-quarter observations, compared to 55,565 observations in our full sample. The results from this reduced sample are qualitatively similar to those shown in Tables 2 through 5. For example, compared to the results shown in Table 5, Panel A, the abnormal returns on the Q4 – Q1 hedge portfolio when sorted by $NC\_ANALYSTS_t$ is 1.84 percent (t-stat = 3.63), compared to 1.87 percent (t-stat = 3.66) for the full sample. We also investigate the time-series pattern of our results to check whether they are driven by a few years. In Figure 1, we plot the average yearly coefficient on $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ in Equation (3). We find that the $NC\_ANALYSTS_t$ effect is positive every year and, except for 2002, the magnitudes of the coefficients are comparable. The coefficient on $COV\_ANALYSTS\_ABSENT_t$ is negative every year except 2005 and the magnitudes are comparable. Overall, we conclude that the effect of our analyst interest variables is consistent over time.

Alternative Explanations: Microstructure Effects and Investor Overreaction

We consider market microstructure effects and investor overreaction as alternative explanations to our return results. Under the microstructure story, firms with changes in market interest, as reflected in changes in analyst coverage and institutional ownership, have sharp stock price movements because of the limited supply of the firm's stock. Such price changes will reverse quickly, suggesting a negative (positive) correlation between $NC\_ANALYSTS_t$ ($COV\_ANALYSTS\_ABSENT_t$) and stock returns in later quarters. Under the investor overreaction story, investors and analysts overreact to firm fundamental information, resulting in a temporary change in analyst interest and stock prices. This story also suggests a negative (positive) correlation between $NC\_ANALYSTS_t$ ($COV\_ANALYSTS\_ABSENT_t$) and stock returns in later quarters. To examine these two alternative explanations, we test (using regression Equation (3)) whether $NC\_ANALYSTS_t$ and $COV\_ANALYSTS\_ABSENT_t$ are correlated with stock returns from month *m*+4 to month *m*+6 (RET_{q+2}) and from month *m*+7 to month *m*+9 (RET_{q+3}), where *m* is the month in which the conference call occurs. If either story is true, then we should find a negative coefficient on $NC\_ANALYSTS_t$ and a positive coefficient on $COV\_ANALYSTS\_ABSENT_t$. Table 7, Panel A reports the results; the coefficient on $NC\_ANALYSTS_t$ remains statistically positive in the RET_{q+2} regression and becomes marginally positive in the RET_{q+3} regression, inconsistent with a reversal effect. In contrast, the coefficient on $COV\_ANALYSTS\_ABSENT_t$ is marginally positive in the RET_{q+2}

[21] We thank an anonymous reviewer for this suggestion.

FIGURE 1
The Time-Series Pattern of the Coefficients on *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT*



This figure charts the average quarterly coefficients on *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* in regression Equation (3) for each year in the sample period. For each quarter, we run the regression of future three-month stock return (*RET*) on *NC_ANALYSTS*, $COV\_ANALYSTS\_ABSENT_t$, *ENEWS*, *SIZE*, *BTM*, and $RET_{m-11,m-1}$ (Model 1 of Table 4).

All variables are defined in Appendix A.

regression, suggesting a small reversal, while the coefficient is negative and insignificant in the RET_{q+3} regression. Overall, we find that neither microstructure effects nor investor overreaction drive our main return results, especially for those documented with increases in analyst interest ($NC\_ANALYSTS_t$).

Robustness Check: Subsamples Based on Information Environment

We partition our sample into three groups based on the number of analysts covering the firm (NAN_t) to examine whether the effects of analyst interest are stronger for firms with poorer information environments. The results are presented in Table 7, Panel B; the coefficients on $NC\_ANALYSTS_t$ are statistically positive across all terciles, but statistical significance declines monotonically from the bottom to the top tercile. As for $COV\_ANALYSTS\_ABSENT_t$, the coefficients are significantly negative in the bottom and middle terciles and insignificant in the top tercile. Statistical significance also declines monotonically from the bottom to the top tercile. When performing statistical tests for coefficient estimates across terciles, we find that $COV\_ANALYSTS\_ABSENT_t$ has significantly more negative coefficients in the bottom tercile than in the other two terciles, while the differences in coefficients on $NC\_ANALYSTS_t$ are statistically insignificant across three terciles. Overall, the evidence weakly supports the idea that the predictive power of analyst interest variables on future returns is stronger for neglected firms.

TABLE 7

Robustness Checks: Regressions of Future Stock Returns

Panel A: Regressions of Subsequent Stock Returns by Different Windows

Dependent Variable:	(1) RET_{q+1} (as Reported in Column (2) of Table 4)	(2) RET_{q+2} (Two-Quarter -Out Returns)	(3) RET_{q+3} (Three-Quarter -Out Returns)
Intercept	0.050	0.045	0.087
	(1.20)	(1.03)	(1.73)
$NC\_ANALYSTS_t$	0.023***	0.014**	0.010*
	(2.72)	(1.75)	(1.43)
$COV\_ANALYSTS\_ABSENT_t$	−0.005***	0.003*	−0.003
	(−2.18)	(1.39)	(−1.11)
$ENEWS_t$	3.27***	0.144	−0.114
	(8.69)	(0.64)	(−0.31)
$SIZE_t$	−0.006	−0.005	−0.007
	(−1.62)	(−1.27)	(−1.74)
BTM_t	0.009	0.001	−0.011
	(0.89)	(0.14)	(−1.13)
$RET_{m-11,m-1}$	−0.019	−0.040	−0.034***
	(−1.34)	(−1.60)	(−2.65)
$CNAN_t$	−0.012*	−0.006	−0.006
	(−1.80)	(−0.95)	(−0.76)
$CNII_t$	0.014	−0.008	0.000
	(0.79)	(−0.45)	(0.02)
$CTURNOVER_t$	0.007	0.053	0.016
	(0.22)	(1.25)	(0.58)
Adj. R^2	0.057	0.032	0.033

Panel B: Regressions of Subsequent Three-Month Stock Returns (*RET*) by Analyst Coverage

	Pred. Sign	Bottom NAN_t Tercile		Middle NAN_t Tercile		Top NAN_t Tercile	
		(1)	(2)	(3)	(4)	(5)	(6)
Intercept		0.138**	0.135**	0.028	0.031	0.033	0.031
		(2.01)	(2.01)	(0.61)	(0.68)	(0.98)	(0.95)
$NC\_ANALYSTS_t$	+	0.021***	0.020**	0.021**	0.024*	0.025*	0.024*
		(2.33)	(2.18)	(1.88)	(1.58)	(1.57)	(1.52)
$COV\_ANALYSTS\_ABSENT_t$	−	−0.015**	−0.015**	−0.004*	−0.005*	−0.002	−0.002
		(−2.14)	(−2.17)	(−1.39)	(−1.52)	(−1.01)	(−0.96)
$ENEWS_t$	+	2.96***	3.02***	4.47***	4.49***	3.64***	3.77***
		(5.59)	(5.76)	(6.51)	(6.54)	(6.16)	(6.07)
$SIZE_t$		−0.019**	−0.019**	−0.003	−0.004	−0.003	−0.003
		(−2.40)	(−2.34)	(−0.75)	(−0.81)	(−1.02)	(−1.00)
BTM_t		0.012	0.008	0.009	0.009	−0.006	−0.006
		(0.90)	(0.58)	(0.66)	(0.69)	(−0.55)	(−0.54)
$RET_{m-11,m-1}$		−0.016	−0.017	−0.015	−0.011	−0.014	−0.014
		(−1.36)	(−1.25)	(−0.89)	(−0.75)	(−0.86)	(−0.89)

(continued on next page)

TABLE 7 (continued)

	Pred. Sign	Bottom NAN_t Tercile (1)	Bottom NAN_t Tercile (2)	Middle NAN_t Tercile (3)	Middle NAN_t Tercile (4)	Top NAN_t Tercile (5)	Top NAN_t Tercile (6)
$CNAN_t$			−0.003		−0.022*		−0.000
			(−0.29)		(−1.72)		(−0.03)
$CNII_t$			0.004		−0.008		0.025
			(0.12)		(−0.28)		(0.77)
$CTURNOVER_t$			−0.021		0.043		−0.005
			(−0.30)		(0.88)		(−0.19)
Adj. R^2		0.060	0.062	0.060	0.068	0.057	0.060

*, **, *** Indicate significantly different from zero at the 0.10, 0.05, and 0.01 level, respectively, using a one-tailed test. The Fama and MacBeth (1973) t-statistic is presented in parentheses under the estimated coefficient.

Panel A presents results of regressions of future stock returns (*RET*) on the number of non-covering analysts ($NC\_ANALYSTS_t$) and covering analysts who are absent ($COV\_ANALYSTS\_ABSENT_t$) from a firm's earnings conference call, as well as on control variables. The results are comparable to those presented in Table 4, except that in columns (2) and (3), stock returns are measured over the three-month windows [*m*+4, *m*+6] and [*m*+7, *m*+9], respectively, where *m* is the month in which a firm's conference call occurred. Panel B presents results of regressions of future stock returns (*RET*) on $NC\_ANALYSTS_t$, $COV\_ANALYSTS\_ABSENT_t$, and control variables. The results are comparable to those presented in Table 4, except that the sample is partitioned into terciles based on firms' level of analyst coverage (NAN_t) prior to the conference call date.

All variables are defined in Appendix A.

Robustness Check: The Effect of Transaction Costs

While our analyses of returns show that changes in analyst interest predict future stock returns and predictive power is stronger for neglected stocks, we do not take into account transaction costs. Transaction costs include the bid-ask spread, commissions paid to a broker, and the price impact of the buy or sell order. Broker commissions have been declining over the past 15 years, with many discount brokers offering very low or even zero commissions for an unlimited number of shares per trade. The price impact depends on the trade size and can be substantial for large trades of small-cap stocks. Our conversation with a portfolio manager indicates that total transaction costs are about 15 basis points for large-cap stocks and 70 basis points for small-cap stocks (Russell 2000) for a portfolio of $500 million. As institutional investors typically incorporate multiple signals in their trading strategies (e.g., 10–12 signals in our portfolio manager's case), transaction costs are shared by these multiple signals, further lowering the transaction costs to implement the strategies. Within this context, we surmise that transaction costs can reduce the profitability of the *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* strategies if traded alone, and that the strategies are potentially profitable only to institutional investors with low transaction costs and careful execution. However, *NC_ANALYSTS* and *COV_ANALYSTS_ABSENT* can add significant value to a portfolio that trades on multiple signals and spreads transaction costs across these signals.

Robustness Check: IPO Firms and Fourth-Quarter Effects

We examine whether the positive association between *NC_ANALYSTS* and future analyst coverage is driven by firms that recently had an IPO. Relative to firms that have been public for many years, recent IPO firms may exhibit larger sequential increases in analyst participation in conference calls and analyst initiations during the first few quarters after their IPOs. We repeat our analysis after excluding all firm-quarters where a firm's IPO occurred within the past 12 months, which reduces the sample size by 5,394 firm-quarters (10 percent of total firm-quarters). We find

that the results (not tabulated) and inferences are virtually unchanged from the main results discussed in Section IV.

We also examine whether there is a fourth-quarter effect driving our main results. It is possible that analyst interest in a firm's fourth fiscal quarter is higher because results are aggregated for the full year, news is delayed until the fourth quarter (Mendenhall and Nichols 1988), or analysts can ask questions about the next fiscal year. We investigate this possibility and its potential influence on the main results in two ways. First, we compute the mean and median number of analysts who ask a question during a conference call by quarter. We find that the mean is 5.1 and median is 5.0 in the fourth quarter, the same as in each of the first three quarters. Thus, it does not appear that the average level of analyst interest is significantly different in the fourth or any other quarter. Second, we rerun regression Equations (1) through (3) with the inclusion of quarter fixed effects and find that the results are very similar to those shown in Section IV. In summary, we believe that our main results are not driven by fourth-quarter effects.

VI. CONCLUSION

In this study, we posit that analyst interest is an early indicator of a firm's future fundamentals, capital market activities, and stock price movements. Changes in analyst interest capture changes in firm fundamentals that are not yet reflected in financial statements. Our measure of increasing analyst interest in a firm is based on non-covering analysts who participate in the firm's earnings conference calls, which we argue is an observable aspect of analysts' due diligence prior to coverage initiations. Conversely, our measure of decreasing analyst interest (or disinterest) is based on covering analysts who are absent from the firm's conference calls.

We find that increases in analyst interest are positively associated with future fundamental changes, such as earnings growth and sales growth, after controlling for observable financial statement variables and other determinants. Increases in analyst interest are also positively associated with future capital market activities, such as analyst coverage, institutional ownership, and trading volume turnover. In contrast, we find weaker results based on decreases in analyst interest, which we attribute to greater noise in our measure of analyst disinterest. In our analyses of future stock returns, we show that both increases and decreases in analyst interest predict stock returns over the next three months and that a hedge portfolio yields a significant abnormal return. Overall, our study shows that analyst interest is a novel and early indicator of future firm fundamentals and capital market consequences. Our focus on analyst interest prior to coverage decisions (i.e., due diligence) also contributes to our general understanding of the role that analysts play in the capital markets.

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APPENDIX A

Definitions of Variables

Variable	Definition	Data Source
NUMCALLERS	Number of analysts that asked a question on the firm's conference call.	Thomson StreetEvents
NC_ANALYSTS	Number of analysts that asked a question on the firm's conference call, but did not cover the firm as of the date of the conference call, scaled by *NUMCALLERS*.	Thomson StreetEvents
COV_ANALYSTS	Number of analysts that asked a question on the firm's conference call and covered the firm as of the date of the conference call, scaled by *NUMCALLERS*.	Thomson StreetEvents
COV_ANALYSTS_ABSENT	Number of analysts that covered the firm as of the date of the conference call, did not ask a question on the conference call, but did ask a question on the previous conference call, scaled by *NUMCALLERS*.	Thomson StreetEvents
ΔEPS	Seasonal difference in earnings per share (diluted) excluding extraordinary items, scaled by the firm's stock price on the last day of the fiscal quarter ended prior to the conference call.	Compustat
SGROWTH	Seasonal percentage change in quarterly sales.	Compustat
NAN	Number of analysts that covered the firm as of the date of the conference call, defined as the number of analysts that issued an earnings estimate any time between the previous conference call date and one day before the conference call.	I/B/E/S

(continued on next page)

APPENDIX A (continued)

Variable	Definition	Data Source
CNAN	Percentage change in the number of analysts that covered the firm, defined as the number of analysts that issued an earnings forecast any time between the date of the conference call and the next conference call, divided by *NAN*, minus one.	I/B/E/S
NII	Number of institutions that owned the firm's stock as of the most recent calendar quarter ended prior to the conference call.	Thomson Reuters 13F Database
CNII	Percentage change in the number of institutions that owned the firm's stock from the calendar quarter ended prior to the conference call to the calendar quarter ended after the conference call.	Thomson Reuters 13F Database
CTURNOVER	Change in average daily turnover (volume divided by shares outstanding) from the 90 days before to the 90 days after the conference call, expressed in percentage terms.	CRSP
RET	Return over the three-month period [m+1, m+3], where m is the month in which the conference call occurs.	CRSP
SIZE	Natural logarithm of the market value of equity, measured as of the most recent fiscal quarter ended prior to the conference call.	Compustat
ROA	Income before extraordinary items divided by total assets, measured as of the most recent fiscal quarter ended prior to the conference call.	Compustat
BTM	Book value of equity divided by market value of equity, measured as of the most recent fiscal quarter ended prior to the conference call.	Compustat
LEVERAGE	Book value of debt divided by book value of equity, measured as of the most recent fiscal quarter ended prior to the conference call.	Compustat
PASTRET	Size-adjusted return (raw return less return of the corresponding size decile) for the period [−91, −1] where day 0 is the date of the conference call.	CRSP
VOLATILITY	Standard deviation of daily size-adjusted returns for the period [−91, −1] where day 0 is the date of the conference call.	CRSP
$ENEWS_t$	Actual reported EPS for quarter t minus the corresponding mean consensus forecast prior to the conference call, scaled by stock price on the consensus forecast date.	I/B/E/S
$RET_{m-1,m-11}$	Past 11-month return from month $m-11$ to month $m-1$, where m is the month in which the conference call occurred.	CRSP

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Accounting Credibility and Liquidity Constraints: Evidence from Reactions of Small Banks to Monetary Tightening

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ABSTRACT: This study examines the relationship between accounting credibility and firms' ability to fund their investments. Theory suggests that credible reporting resulting from external audits enables firms to attract external funds needed for their investments. The tests exploit monetary policy tightening that creates a liquidity shortage for banks, which, in turn, either requires banks to raise additional funds to restore liquidity or forces them to restrict their investments in the form of lending. Studying small non-public banks for which external audits are voluntary, I find that audited banks can better access funds during periods of monetary tightening than unaudited banks. As such, adverse liquidity shocks impede the lending of audited banks less. Overall, these findings present new evidence on how accounting credibility affects firms' ability to invest.

Keywords: *external audits; accounting credibility; external financing; investment; banks.*

Data Availability: *The data are available from the sources indicated in the text.*

I. INTRODUCTION

Investment policies and value creation can often be distorted by problems arising from information asymmetry between firms and outside investors (Stein 2003). Of particular interest to accounting researchers is whether and how financial reporting affects firms' investment policies. One way through which financial reporting can facilitate investment is to allow liquidity-constrained firms to attract external financing and, thus, make investments that they would

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otherwise forgo. Biddle and Hilary (2006), for example, find that investments of firms with high reporting quality are less affected by the availability of internally generated cash flows. While prior studies have offered valuable insights, additional research is needed to determine "whether the negative relation between reporting quality and underinvestment is due to firms' ability to raise debt and/or equity capital" (Biddle, Hilary, and Verdi 2009, 129).

In this spirit, this study tests whether credible reporting enables firms to raise external funds and, thus, sustain their investments during liquidity shortages. Such a setting allows me to test directly the proposed links between reporting credibility and investments.

After a contraction of monetary policy by the U.S. Federal Reserve (hereafter, the Fed), banks' ability to use insured deposits as a funding source will be directly compromised. Banks can try to restore their liquidity with alternate *uninsured* financing such as large certificates of deposits (CDs), but access to uninsured financing may be restricted by investor uncertainty about the issuing bank's financial standing. Building on this observation, a series of economic studies documents that liquidity losses due to monetary contractions can lead to suppression of bank investments in the form of lending. This effect is particularly pronounced among small banks, which are, perhaps, the banks most subject to information problems (Kashyap and Stein 2000). Based on these findings, I conjecture that small banks with higher levels of reporting credibility can attract more uninsured financing and better maintain their lending levels following exogenous liquidity losses triggered by monetary tightening. This setting provides a direct and immediate link between availability of financing and investment, enabling a close examination of the role of reporting credibility in firms' ability to fund their investments.

An interesting aspect of the setting is the reporting environment for small banks. Small banks with less than \$500 million in total assets are an important group of financial companies[1] and specialize in making relationship-based loans to "informationally opaque" borrowers, such as start-up firms and small businesses (Keeton 2003). Their specialization creates considerable information asymmetry between small banks themselves and external investors. To mitigate this problem, many small, non-public banks *voluntarily* engage external auditors to attest to the reliability of their financial reports. However, because banks' financial reporting is subject to regulatory oversight by bank supervisors, which serves as an alternative source of monitoring, it is unclear whether auditor monitoring can add reporting credibility as effectively as it does for unregulated firms.[2] Can audited financial statements help small banks to attract loanable funds? To illuminate these issues, I test whether the benefits of being audited are evident in small banks' responses to liquidity losses caused by monetary tightening.

Compared with unaudited small banks, I find that audited small banks enjoy greater access to uninsured financing to counteract Fed-induced liquidity losses. Accordingly, adverse liquidity shocks impede the lending of audited small banks less. These results hold under different tests that address the endogenous choice of an audit. Studying the subsample of banks that change audit status over time, I also find that banks can better withstand liquidity losses when they are audited.

I conduct cross-sectional analyses to strengthen these inferences. I expect the positive effect of audits to arise mainly among banks that must rely on uninsured financing as the marginal source of funds. Prior studies find that banks with fewer liquid assets have greater difficulty restoring liquidity by sales of assets and, hence, must rely more on uninsured financing to protect their loan growth following monetary tightening (Kashyap and Stein 2000). Thus, I expect and find that the effect of audits is limited to banks with fewer liquid assets.

I use a dynamic capital market setup to document an important mechanism through which firms benefit from auditor assurance during periods of liquidity shortage. With an external audit, firms can better *increase* the funds available to them when they need the funds the most. The

[1] Based on information from the Reports of Condition and Income database, more than 80 percent of U.S. banks in 2008 could be classified as small banks, and their aggregate outstanding lending exceeded \$650 billion.

[2] See, for example, Chang, Dasgupta, and Hilary (2009) and Minnis (2011) for studies that examine unregulated firms.

empirical demonstration of the financing effect of external audits is consistent with a causal relation in which audits enhance the credibility of financial reports, which, in turn, enables firms to attract funds required for their investments.

My inferences are further strengthened by the focus on a single industry segment where the firms have similar operations and face the same liquidity shocks. In contrast, prior research, which largely depends on cross-sectional tests studying firms from diverse industries, is more likely to suffer from correlated omitted variables (Cassar 2011).

My results also have implications for understanding banking and its impact on the economy. Banks play a central role in channeling funds from savers to businesses, but this role hinges on their ability to attract loanable funds (Houston, James, and Marcus 1997). Thus, banks' financial flexibility is crucial in the capital allocation in the overall economy. My results indicate that reporting credibility can be a key component of that flexibility. To my knowledge, this study is among the first to directly test this important role of reporting credibility in financial intermediation. At the same time, this study indicates that improved financial flexibility due to credible reporting can weaken the effectiveness of liquidity shocks triggered by monetary policy. Prior studies have discussed this potential effect of bank transparency, including Kashyap and Stein (2000) and Holod and Peek (2007), but little direct evidence on such effects has emerged. Thus, this study also illuminates the transmission of monetary policy and its ability to influence the overall economy.

Next, Section II reviews prior research and develops the hypotheses. Section III presents the research design. Section IV discusses sample selection, descriptive statistics, and the main results. Section V reports additional tests. Section VI concludes.

II. PRIOR RESEARCH AND HYPOTHESIS DEVELOPMENT

Accounting Disclosure, External Financing, and Firm Investment

Prior studies examining the effects of accounting disclosures on firm investments suggest two roles for disclosures. First, reporting quality can reduce overinvestment problems by mitigating information asymmetries that cause moral hazard (e.g., Biddle and Hilary 2006; Hope and Thomas 2008; McNichols and Stubben 2008; Biddle et al. 2009; Francis and Martin 2010). Second, it can reduce adverse selection, thereby increasing the availability of external financing and mitigating underinvestment problems. This study builds on this second stream of literature.

Prior evidence shows that the investments of firms with high reporting quality are less affected by the availability of internally generated cash flows (Biddle and Hilary 2006), consistent with these firms having more flexibility in obtaining alternate sources of funds. Biddle et al. (2009) find a positive association between reporting quality and investment among firms subject to underfunding, consistent with reporting quality enabling firms to attract funds for investments that they would otherwise forgo. Chen, Hope, Li, and Wang (2011) show that private firms in emerging markets are less likely to invest below predicted levels if they have higher reporting quality, and this finding is more pronounced among firms that finance more through bank loans. Overall, these results offer valuable insights into the mediating effect of financial reporting on the link between financing and investment. However, none of these studies directly tests whether the negative relation between reporting quality and underinvestment is due to firms' ability to raise external funds, the issue I examine. Studying 15 oil firms whose operating cash flows and, hence, ability to fund investments were negatively affected by the 1986 oil price decrease, a concurrent working paper finds that the firms with higher AIMR (Association for Investment Management and Research) disclosure ratings before the oil price shock are associated with a lower decrease in capital investments afterward (Frederickson and Hilary 2007). In contrast to that study, I use a large sample of financial firms to examine whether audited financial statements can facilitate their investments through increasing availability of external funds.

Monetary Policy, Bank Liquidity, and Bank Lending

To outline the rudiments for my study, I provide a simplified example that illustrates a bank's response to monetary tightening.[3] The Federal Reserve (Fed) typically implements monetary tightening through a large-scale open-market sale of government securities. Consider a hypothetical case where the Fed sells $10 million of securities. Before the sale takes place, the financial position of the bank of the purchasers is as follows:

Assets		Liabilities and Equity	
Reserves	$10 million	Reservable Deposits	$100 million
Loans	$100 million	Bank Capital	$10 million

The bank is assumed to hold most of its assets in the form of loans. It keeps the other $10 million of reserves in the form of deposits at the Fed because U.S. banks are legally required to hold reserves against the funds they acquire. The size of required reserves is determined by applying legal reserve ratios to liabilities subject to reserve requirements. For example, if the legal reserve ratio is 10 percent and the bank holds $100 million of reservable deposits, then its required reserves is $10 million.

As a result of the Fed's sale, the bank loses $10 million of reservable deposits. This occurs when the purchasers withdraw $10 million from the bank to pay the Fed for the securities. When the Fed receives $10 million in checks drawn on the bank, the Fed deducts the proceeds from the bank's deposits with it. Thus, the bank's reserves fall by $10 million and its financial position becomes:

Assets		Liabilities and Equity	
Reserves	$0 million	Reservable Deposits	$90 million
Loans	$100 million	Bank Capital	$10 million

After the $10 million deposit outflow, the bank has a reserve deficiency of $9 million (10 percent of $90 million). To raise reserves, the bank can issue debt, such as large certificates of deposits (CDs), that requires lower reserves. However, nonreservable capital providers lack the federal insurance protection provided to reservable depositors, and investors' concerns about bank quality will limit the bank's ability to obtain financing from these alternate sources (Lucas and McDonald 1992). If the bank can raise only $1 million of large CDs due to restricted market access, then it will contract lending by $8 million to meet its obligatory level of reserves, for example, by selling loans or applying loan payments to reserves. Because contracting lending is considered the "costliest way of acquiring reserves when there is a deposit outflow" (Mishkin 2006, 228),[4] banks generally raise uninsured financing first.

The bank may also want to raise insured deposits at the expense of other banks. However, insured depositors often choose their bank based on service and relationship factors, which banks cannot adjust quickly (Office of the Comptroller of the Currency [OCC] 2012).[5] Thus, access to uninsured financing "is a key factor in determining the extent to which a bank must adjust its loan portfolio when monetary policy is tightened" (Peek and Rosengren 2013, 10).

Based on the above intuition, analytical studies develop adverse selection models that show the direct impact of a Fed-induced tightening on bank lending (Stein 1998). Consistent with these

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3 Mishkin (2006) provides further details on how monetary policy tightens money supply in the whole banking system through the process commonly known as multiple deposit contraction.

4 For example, Mishkin (2006, 229) notes that "this is likely to antagonize customers whose loans are not being renewed . . . they are likely to take their business elsewhere in the future, a very costly consequence for the bank."

5 In addition, Feldman and Fettig (1998) note that raising rates to attract insured deposits would also increase the cost of the bank's existing deposit base, so banks are reluctant to use insured deposits as a marginal source of funds.

studies' predictions, Kashyap and Stein (1995, 2000) find that lending by small banks, which are more subject to information problems, is more negatively affected by monetary tightening than large bank lending.[6] This negative effect is more pronounced if a bank lacks liquid assets to sell or pledge and, hence, must rely on uninsured financing to restore liquidity. Subsequent research finds other cross-sectional bank differences that can also explain the differential sensitivities of lending to monetary tightening. For example, Campello (2002) and Ashcraft (2006) find that lending by independent banks unaffiliated with multibank holding companies is more sensitive to tightening because these banks lack access to internal capital markets. Kishan and Opiela (2000, 2006) similarly find a higher lending sensitivity to tightening for banks with lower equity levels because risky banks are less able to attract uninsured financing. Controlling for these bank characteristics, Holod and Peek (2007) show that the lending by small, non-public banks is most sensitive to monetary tightening. The authors conjecture that limited financial disclosures by these banks contribute to their findings, but they do not test this conjecture.

Hypotheses Development

Financial statements provide depositors and other funds suppliers with an important source of information concerning a small bank's financial position. However, accounting reliability will be low if managers abuse their discretion in accounting policies and estimates. Such abuse was highlighted in the many cases of small bank failures in the 1980s and early 1990s, where misleading financial statements helped to hide the losses of failed banks, obscuring the decline of the banks' financial health (General Accounting Office [GAO] 1991). After these failures, "[d]epositors generally became more selective in their choice of banks" (FDIC Federal Deposit Insurance Corporation [FDIC] 1998, 540). Consistent with this observation, it is often suggested that "reliable financial reports are necessary for [small banks] to raise capital" (e.g., *Federal Register* 1999, 57095).

Monitoring by bank supervisors over financial reporting occurs in the context of periodic onsite safety and soundness examinations. These examinations happen at each bank at least once every 18 months. The aim is to evaluate the financial health of the bank and provide early identification of both problems and remedies. Bank examiners assess whether overall management quality is sufficient for the nature and scope of the bank's business, especially the high-risk areas relating to lending. After evaluating the bank's credit controls and loan quality, examiners must verify a bank's financial disclosures and determine whether its allowance for loan losses is adequate (Federal Reserve Board of Governors [FRBOG] 1999). As part of their work, bank examiners review work papers that support the information disclosed in the bank's financial reports and verify whether the disclosures agree to the bank's accounting systems. Gunther and Moore (2003) show that regulatory reviews occasionally prompt accounting restatements that correct loan loss underreporting.

To the extent that regulatory oversight works, it assures a minimal level of disclosure quality. If investors believe such efforts are effective, then banks' voluntary mechanisms for safeguarding reporting reliability are unlikely to have significant incremental benefit in terms of reducing information asymmetry. However, regulatory reviews are not without limitations. For example, examiners have difficulty measuring banks' loan loss exposures and sometimes agree with overstatements of asset values made by banks that later failed (GAO 1991). Such failures can be partly attributable to regulators' resource constraints, which reduce the effectiveness of their oversight (FDIC 1997, Chapter 12). Separately, examiners do not opine on the fair presentation of the bank's financial reports, and they do not release the examination findings to the public. Given these limitations, investors may welcome

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[6] Studies that survey managers of small banks suggest that deposit outflows can often force these banks to "curtail lending to creditworthy customers" (Harvey and Spong 2001, 39).

additional independent monitoring and assurance. Consistent with this demand, many small banks *voluntarily* engage external auditors to attest to the reliability of their financial reports.

Section 36 of the Federal Deposit Insurance (FDI) Act, as implemented by FDIC Regulation 12, Code of Federal Regulations Part 363, requires banks with $500 million or more in total assets at the beginning of their fiscal year to have an annual audit conducted in accordance with generally accepted auditing standards (GAAS) by an independent public accountant. This requirement, together with others specified in Section 36 of the FDI Act, is "intended to mitigate information asymmetries between banks and their stakeholders by improving the quality and oversight of financial reporting" (LaFond and You 2010, 76). However, due to high compliance costs, banks below the $500 million threshold are not subject to Section 36. Thus, small banks can choose one of several low-cost alternatives, including a review or other agreed-upon procedures.

Because an external audit involves significantly more extensive planning and procedures to verify the information provided in accounting reports than other alternatives (Kohlbeck 2005; Singh 2007), bank regulators routinely identify an audit as the preferred choice to enhance the reliability of financial reporting (*Federal Register* 1996, 32439). Similarly, in its report to Congress on failed banks, the GAO (1991, 8) stresses that "without the discipline of an audit, troubled institutions are more able to cover up their financial difficulties." Consistent with these arguments, Gunther and Moore (2003) find that external audits can prompt accounting restatements that correct loan loss underreporting, and the effect is incremental to regulatory reviews. Dahl, O'Keefe, and Hanweck (1998) show that after controlling for bank performance, external audits are associated with greater loan loss provisions, consistent with more conservative provisioning at audited banks. In unreported tests, I confirm that audited small banks exhibit more timely recognition of loan losses in earnings than other small banks, consistent with effective external auditor monitoring.

To the extent that investors believe audited small banks issue more reliable financial disclosures than unaudited banks, they are likely to perceive that these banks have lower information uncertainty and be more willing to provide the required financing. Consistent with this argument, managers of small banks surveyed in Matt (1987, 95) expressed the following when they were asked about the potential benefits of having an audit: "[T]he public will feel better about the bank when they see that an outside firm has reviewed it ... They like to see certified financial statements before they put their money into an operation." Compared with unaudited small banks, I predict that audited small banks enjoy greater access to uninsured liabilities to counteract liquidity losses caused by monetary tightening. Stated in alternative form, the hypothesis is as follows:

H1: The growth of uninsured liabilities during periods of monetary policy tightening is higher for audited banks than for unaudited banks.

If audits allow small banks to restore the immediate liquidity shortfall caused by monetary tightening using uninsured liabilities, then audited small banks are less likely to be forced to curtail their lending compared with unaudited banks. This suggests an indirect effect of audits on loan growth through growth in uninsured liabilities. Audits can also affect loan growth through banks' expectations about future liquidity constraints. With greater access to alternate financing sources, audited banks are likely to have fewer concerns about future liquidity problems and, hence, be more willing to lend even in a tight money cycle. Both the direct and indirect effects of audits predict that audited banks will maintain their lending levels more effectively than unaudited banks during monetary tightening. Thus, I test the following hypothesis, stated in alternative form:

H2: Suppression of lending during periods of monetary policy tightening is lower for audited banks than for unaudited banks.

While audits are expected to entail benefits, they come with costs. For example, because audit fees contain a fixed component, it is relatively expensive for small banks to obtain an audit

(Kohlbeck 2005). Audit choice also depends on managers' personal beliefs. Some managers might be concerned with unwanted regulatory interventions if regulators use independently audited accounting information to identify weak banks. Having additional monitoring by auditors will limit managers' ability to hide their banks' problems from regulators. Other managers may believe that the added credibility due to voluntary audits is limited because all banks are subject to regulatory reviews. To sum, audit choice likely reflects the differential costs and perceived benefits for each bank. In Section III, I discuss multiple strategies for addressing potential self-selection issues.

Note that the predicted effects of audits are more detectable within small banks that rely more on uninsured liabilities as the marginal source of funds. Prior studies find that banks with fewer liquid assets have greater difficulty restoring their liquidity by sales of assets and, hence, must rely more on uninsured liabilities (Kashyap and Stein 2000). Thus, I expect the predicted effects of audits to be more limited to these banks and conduct additional analyses to strengthen inferences.

III. EMPIRICAL MODEL SPECIFICATION

Empirical Model

I use the following pooled time-series cross-sectional model for the main tests:

$$\begin{aligned} D\_Change_{it} = {} & \alpha + \sum_{j=0}^{5} \beta_j TightMP_{t-j} + \gamma Audited_{it-4} + \sum_{j=0}^{5} \delta_j TightMP_{t-j} \times Audited_{it-4} \\ & + \zeta Bank\text{-}Level\ Control_{it-1} + \sum_{j=0}^{5} \eta_j TightMP_{t-j} \times Bank\text{-}Level\ Control_{it-1} \\ & + \ldots + \sum_{j=1}^{5} \theta_j D\_Change_{it-j} + \sum_{j=0}^{5} \iota_j GDP\_Growth_{t-j} + \sum_{j=0}^{5} \kappa_j CPI\_Growth_{t-j} \\ & + \lambda Basel_t + \sum_{j=1}^{50} \nu_j State_{ij} + \xi Time\ Trend_t + \sum_{j=1}^{3} o_j Quarter_{jt} + \varepsilon_{it}, \end{aligned} \tag{1}$$

where i and t denote the bank and quarter, respectively. In general, the quarterly change in the dependent variable (*D_Change*), which measures changes in uninsured liabilities or lending, is regressed against a set of monetary tightening indicators (*TightMP*), an audit indicator (*Audited*), its interaction with policy variables (*TightMP* × *Audited*), and a set of controls. The focus is on the moderating effect of an audit on banks' financing and lending responses to monetary tightening. This effect is captured by the sum of the coefficients on *TightMP* × *Audited* ($\Sigma\delta$). I explain the regression model further below and provide details of variable measurement in Appendix A.

Dependent Variables

To test for the financing benefits of auditing (H1), I study changes in uninsured liabilities that banks commonly use to adjust liquidity. Large certificates of deposits (CDs) are a particularly important component of these managed liabilities. Large CDs are those issued in denominations above the $100,000 limit for deposit insurance coverage applicable during the sample period. Financial institutions, local authorities, and municipalities often buy large CDs as investments of their idle funds (Mishkin 2006, Chapter 9). These investors routinely use bank accounting information to assess the quality of large CD issuers. For example, common investment policies of credit unions "stipulated that the investment committee had to obtain the most recent annual financial statement and the most recent quarterly financial statement before investing over the $100,000 federally insured limit in any bank that was not among the nation's top 50 banks in asset

size" (American Banker 1986a). Also, money brokers often rely on a bank's accounting information to recommend the bank to their investor clients (American Banker 1986b).

In addition to large CDs, managed liabilities also include subordinated notes and other borrowed money, other forms of uninsured funds whose availability is sensitive to reporting credibility. Other borrowed money includes the borrowing from nonrelated financial institutions, and prior research suggests that external audits can reduce information uncertainty that limits firms' access to these lines of credit (Berger and Udell 1995; Miller and Smith 2002).

Following prior research (e.g., Campello 2002), I use the quarterly change in total loans to assess the investment impact of auditing (H2). To facilitate comparisons of results across the two tests, I scale both changes in managed liabilities and loans by beginning-of-period total loans. The results persist if I compute both dependent variables as percentage growth rates.[7]

Independent Variables of Interest: Monetary Tightening and the External Audit Status

I use the narrative index developed by Boschen and Mills (1995) to identify periods in which contractionary monetary policies have taken place. Contractionary policies are generally motivated by policymakers' desire to reduce inflation. In contrast, expansionary policies are intended to promote real economic growth. Based on the importance that policymakers assign to reducing inflation relative to promoting real growth, Boschen and Mills (1995) classify the policy stance each month into five categories from "strongly contractionary," coded as −2, to "strongly expansionary," coded as 2. A "neutral" policy is coded as 0. Prior studies assessing the validity of the Boschen-Mills index confirm that it reliably measures policy stance (Jefferson 1998), and the index has been applied in various contexts to capture monetary tightening, including Thorbecke (1997), Campello (2002), and Weise (2008).

Figure 1 charts the value of the Boschen-Mills index (solid line, right axis) at each quarter end throughout the sample period (1988: Q1–2007: Q2). The periods of monetary tightening are indicated by the shaded areas. The chart also plots the shares of bank assets funded by insured deposits and managed liabilities for small, domestically chartered commercial banks in the dotted lines, left axis. As expected, monetary tightening reduces banks' use of insured deposits, as shown in the upper dotted line. In line with banks relying more on uninsured liabilities when policy is tightened, there is a corresponding increase in banks' use of managed liabilities. Hence, changes in banks' funding mix are well correlated with contractionary periods in the expected direction. Altogether, there are five separate contractionary cycles throughout the sample period. On average, each cycle lasts for about six quarters, with contractionary policies in each quarter of the cycle.

Studies consistently find delays in banks' responses to monetary contractions (Bernanke and Blinder 1992). To recognize this effect, I follow prior research by allowing a given contractionary quarter to have a prolonged effect for up to five subsequent quarters (Figure 2, Panel A).[8] The cumulative effect can then be gauged from the sum of the coefficients on the tightening indicators (*TightMP*) and a t-test of whether this sum is statistically significant. Figure 2, Panel B provides an alternative interpretation of this sum. In Section IV, I discuss the economic significance of the results using both interpretations.

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[7] When I suggest that the effect of auditing persists, holds, or remains similar, I mean that the sum of the coefficients on *TightMP* × *Audited* is of similar magnitude and remains statistically significant at least at the 5 percent level.

[8] The appropriate number of lags one should include is unclear. Although some studies include as many as eight lags in their analyses (Campello 2002), most studies use four to six lags (Kashyap and Stein 2000). Alternatively, including four or six policy lags provides similar results.

FIGURE 1
Shares of Liabilities in Total Assets versus Boschen-Mills (BM) Index of Monetary Policy Stance

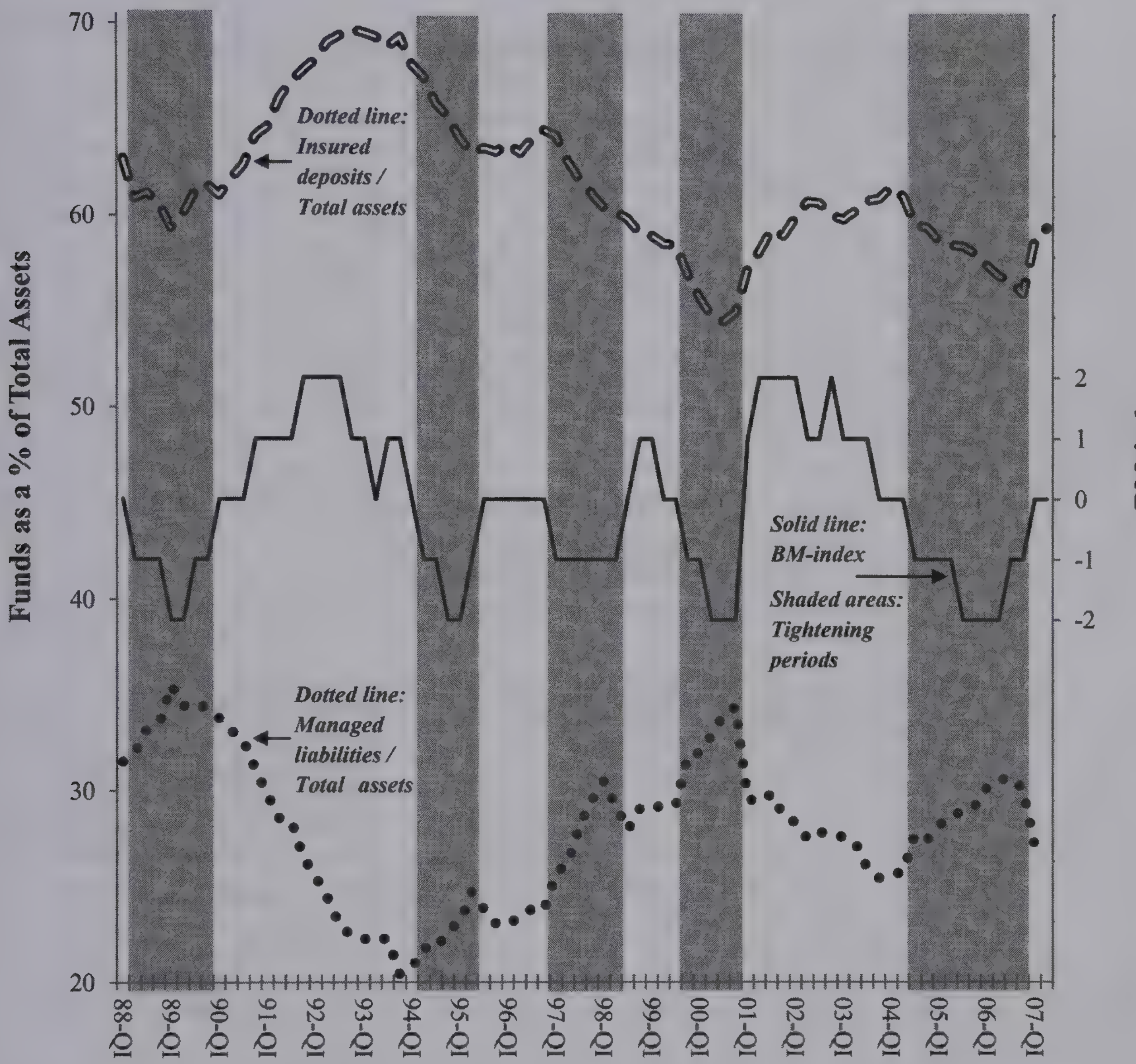


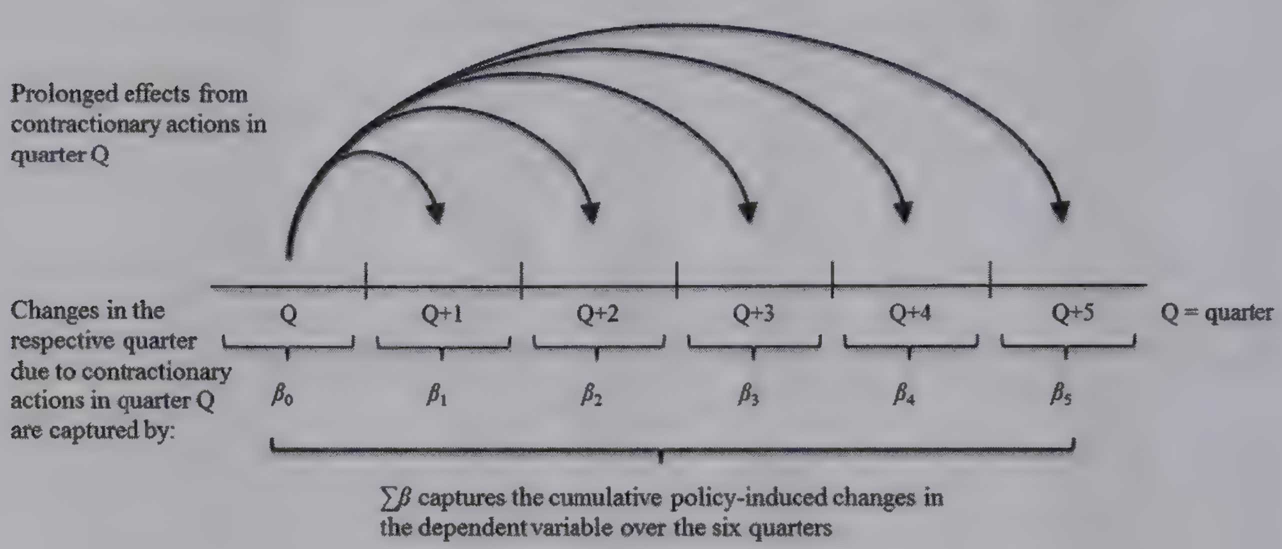
Figure 1 depicts the shares of selected liabilities in total assets in levels (dotted lines; left axis). I compute the share information using aggregate balance sheet data provided by the Federal Reserve for all small, domestically chartered commercial banks (Series H.8). The Federal Reserve defines small, domestically chartered commercial banks as all domestically chartered commercial banks besides the largest 25. I use seasonally adjusted values for (1) total deposits, except large time deposits, and (2) the sum of large time deposits and borrowings to approximate insured deposit and managed liabilities, respectively.

Figure 1 also charts the value of the Boschen-Mills (BM) index (solid line; right axis) at each quarter end throughout the sample period. Boschen and Mills (1995) peruse the policy records of the Federal Open Market Committee and classify the stance of policy into five categories from "strongly contractionary," coded −2, to "strongly expansionary," coded 2. A "neutral" policy is coded 0.

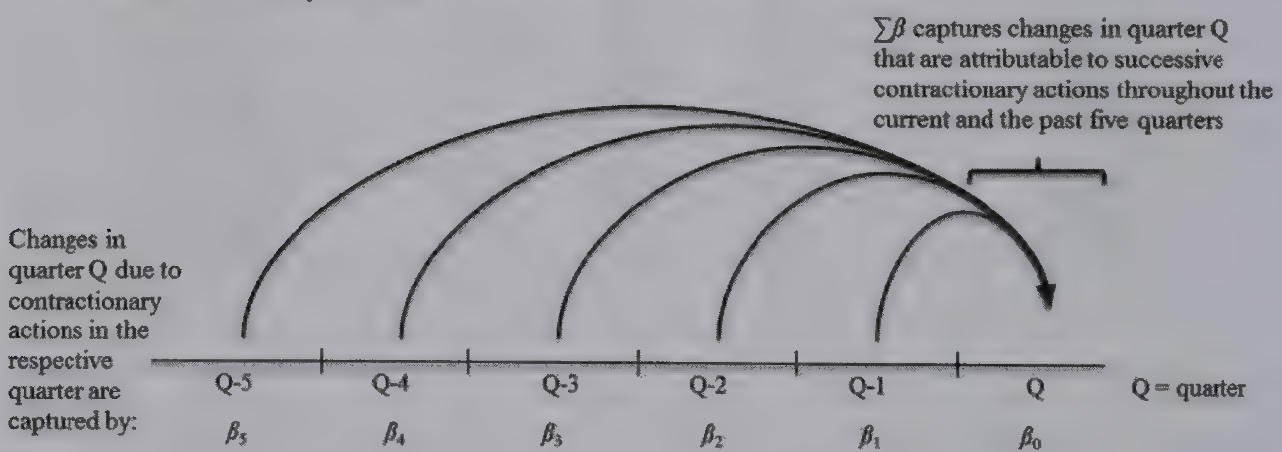
The main question of interest is whether there are significant cross-sectional differences in the way audited and unaudited banks respond to monetary contractions. To allow the responses of audited banks to vary, I interact the policy variables (*TightMP*) with an audit indicator (*Audited*) for banks that received a full-scale financial statement audit in the previous year. If external audits

FIGURE 2
Bank Responses to Monetary Tightening with Lags

Panel A: Cumulative effects of contractionary actions in a given quarter



Panel B: Changes in the current quarter that are attributable to successive contractionary actions



The inclusion of policy lags in Equation (1) allows delays in banks' responses to monetary tightening. There are two interpretations of the sum of the coefficients on *TightMP* (i.e., $\Sigma\beta$). First, assume that the Fed adopts a contractionary policy for one given quarter, shown as quarter Q in Panel A. $\Sigma\beta$ then captures the cumulative effect on a bank of the contractionary policy over the subsequent five quarters.
Alternatively, $\Sigma\beta$ can be interpreted from the perspective of a given quarter that follows multiple contractionary actions in prior periods. Specifically, consider the final quarter of a six-quarter contractionary cycle, shown as quarter Q in Panel B. Under this interpretation, $\Sigma\beta$ captures changes in a bank's managed liabilities (or total loans) in quarter Q that are attributable to successive contractionary actions throughout the current and the past five quarters.

facilitate raising external financing, thereby mitigating the negative policy effects on total loan growth, then I expect the sum of the coefficients on the interaction term ($\Sigma\delta$) to be positive in both tests of funding and lending responses.

Bank-Level Control Variables

The first set of control variables includes bank-level characteristics (*Bank-Level Control*). To isolate the effect of auditing, I include eight variables to control for attributes that may correlate with both the bank's audit status, as well as its policy reactions. I add the natural log of total assets (*Ln*(*TA*)) to account for the differences in financing prospects associated with bank size (Kashyap and Stein 2000).

Next, I exclude small banks owned by a multibank holding company. These banks are relatively unaffected by monetary policy because capital is supplied by other members within the same banking group (Campello 2002; Ashcraft 2006). Also, because these banks are likely to be audited through the consolidated audit of the holding company, excluding them limits the potential confounding affiliation advantages. To preserve sample size, I retain banks that are owned by a one-bank holding company for which assets of the bank subsidiary represent the majority of the holding company's assets. I include an indicator (*OBHC*) for these affiliated banks to account for any potential impacts due to differences in organizational forms. I include an indicator for banks that operate in a metropolitan statistical area (*MSA*) to adjust for potentially greater financing and lending opportunities, as well the availability of an audit in urban areas (Campello 2002). I predict that this interaction term with the policy variables will be positive.

Because banks with a low capital-to-total assets ratio tend to face more financing frictions (Kishan and Opiela 2000), I include an indicator variable for these banks (*LowCap*).[9] Following Ashcraft (2006), I set *LowCap* to 1 if the bank's equity-to-assets ratio is below 6 percent.[10] Using alternate cut-offs does not affect inferences. To further account for differences in risk profile between audited and unaudited banks, I include the ratio of nonperforming loans to total loans (*NPL*) and an indicator variable for banks that report net losses (*Loss*). Similar to *LowCap*, their interaction terms with the policy proxies are expected to be negative.[11]

To control for bank liquidity, I include the ratio of liquid assets to total assets (*Liquid Assets*) (Kashyap and Stein 2000). I also include the bank's ability to generate internal cash flows (*Internal CF*), as proxied by the ratio of (1) the sum of income before extraordinary items and provision for loan losses to (2) beginning-of-period total loans (Houston et al. 1997). The predicted signs for their policy interaction terms are negative (positive) in the test of H1 (H2). *Ceteris paribus,* banks with more internal liquidity are less likely to need external funds, and their loans are less likely to be affected by liquidity shocks.

Economy-Wide and Other Factors

I add five lags of the dependent variable (*D_Growth*) to account for bank-specific unobservables, such as distinct business strategies or growth trends, that affect the bank's current growth. To control for macroeconomic changes and inflation, I add current growth rates of GDP

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[9] Banks' capitalization can be mechanically related to their leverage ratio, which, in turn, can affect growth in liabilities in unexpected ways. Unreported tests show that the results persist if I do not include *LowCap*.

[10] I do not use regulatory capital to define *LowCap* because Call Reports provide the required data only from 1996: Q1 onward. The results hold if I use regulatory capital to define *LowCap* for available observations.

[11] Robustness tests show that the results hold after adding an additional performance control (*ROA*). To keep the regression parsimonious, I do not include this control in the main tests.

(*GDP_Growth*) and the consumer price index (*CPI_Growth*), as well as their five lagged values. This is important because while monetary tightening is associated with periods of inflation, it can also coincide with recessions.[12]

Following Ashcraft (2006), I include an indicator for the period 1988–1992 (*Basel*) to account for changes in bank capital regulations in the late 1980s. I also include a set of indicators for the state in which the bank operates to adjust for local economic conditions. Finally, a linear time-trend and three quarterly indicators are added to control for time and seasonal effects. To keep the regression model parsimonious, I only include the main effects of these variables.

Identification Issues

My tests focus on whether audits allow small banks to access alternative funding sources following exogenous liquidity losses triggered by monetary tightening. This allows me to relate audits to banks' ability to fund lending, assessing more closely the proposed causal relations between reporting credibility, financing, and investment.

However, if audits are associated with omitted factors that cause banks' responses to monetary policy to differ, then my results will be confounded. For example, banks inherently prone to liquidity problems, such as risky banks, have incentives to obtain audits as a precaution.[13] If this tendency is not fully controlled for, then the audit indicator in my tests will capture both the effect of bank risk and that of credible reporting, making it hard to detect the financing benefits of auditing. In principle, net bias due to confounding bank characteristics could be either positive or negative. Thus, I use a number of strategies to deal with this issue. First, the main tests control for factors that may be associated with both bank audit status and monetary policy reactions, as identified in prior studies. Second, I alternatively use both the Heckman (1979) two-stage approach and the predicted probability of an audit as instruments to address potential selection issues. Third, I study the subsample of banks that change audit status over time to mitigate confounding bank-specific factors. Finally, I conduct cross-sectional analyses and test whether the effect of audits mainly arises in the subsample of small banks that are likely to benefit most from auditor assurance. Although there are no tests that can fully address omitted correlated variables, consistent results across these analyses will strengthen inferences.

IV. SAMPLE, DESCRIPTIVE STATISTICS, AND EMPIRICAL RESULTS

Data Sources and Sample

I obtain bank data from the Fed's Report of Condition and Income database or Call Reports.[14] The sample period starts in 1988: Q1, when data on bank audit status became available. To avoid

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[12] Consistent with the arguments of this study, prior studies predict and find credit rationing to increase at the onset of recessions because of tighter monetary policy. For example, Dimitrov and Tice (2006, 1469) contend that "if banks cannot cheaply replace the Fed-induced shortfall in insured deposits at the start of recessions, a contraction in bank reserves is likely to reduce the supply of bank loans."

[13] Prior research finds that bank risk is associated with financing frictions. Also, the lack of internal cash flows can lead banks to reduce lending due to liquidity constraints (Houston et al. 1997). If audits can increase a bank's access to external financing, then risky banks have incentives to obtain audits as a precaution. Consistent with this argument, the audit choice model in Appendices B and C shows that risky banks, such as banks less able to generate internal cash flows, loss-making banks, and banks with greater ROA volatility, are more likely to obtain an audit.

[14] Every national bank, state member bank, and insured nonmember bank is required by the Federal Financial Institutions Examination Council (FFIEC) to file a Call Report as of the close of business on the last day of each calendar quarter. Call Reports are widely used by regulators and the public in monitoring. Unless otherwise instructed, banks must provide financial data that are prepared in accordance with generally accepted accounting principles (GAAP).

TABLE 1

Sample Construction

		Sample Size
Total number of bank-quarters in the period 1988: Q1–2007: Q2		839,552
Less:		
Various bank types, including (1) entities other than FDIC-insured commercial banks,[a] (2) foreign-owned banks, (3) banks inactive in the loan market,[b] (4) credit card banks,[c] (5) banks subject to special analysis by regulators, (6) publicly traded banks,[d] (7) banks with total assets exceeding $500 million, and (8) banks affiliated with multi-bank holding companies[e]	369,434	
Bank-quarters in which a merger occurs, observations in the first three years of a bank's operations, and observations with non-positive total assets	19,098	
Observations with missing audit indicator, or missing required financial data	25,316	
Outliers[f]	3,102	(416,950)
Final sample of bank observations		422,602

[a] I use deposit insurance status and entity type to identify FDIC-insured banks.
[b] These include banks with a loans-to-assets ratio below 10 percent.
[c] Credit card banks include all banks that have a value of credit card loans to total loans exceeding 50 percent.
[d] Following Holod and Peek (2007), publicly traded banks include (1) all stand-alone banks whose equity is publicly traded, and (2) all other banks that are indirectly publicly traded through their parent bank holding companies.
[e] I assume affiliation with a multi-bank holding company if the bank is controlled by a direct or regulatory holder and that holder controls more than one bank.
[f] Similar to previous studies (e.g., Campello 2002), bank-quarters with total asset growth greater than 50 percent are removed.
The sample is collected from the Federal Reserve's Call Report database, including 10,587 banks and a total number of 422,602 bank-quarter observations in the period 1988: Q1–2007: Q2. The table summarizes the sample selection process.

the impact of the financial crises, I measure bank audit status until December 31, 2006, with banks' responses to monetary policy being assessed until 2007: Q2. The sample period covers 33 contractionary quarters clustered in five separate tightening cycles, as shown in Figure 1.

Table 1 summarizes the sample selection. The initially available observations include 839,552 bank-quarters. To create a broadly homogenous sample, I exclude entities other than FDIC-insured commercial banks. Foreign banks, banks inactive in the loan markets, credit card banks, and banks subject to special analysis by regulators are removed because of their different operations and regulatory supervision. I further exclude all publicly traded banks and banks with total assets greater than $500 million because they have mandatory audit requirements. Banks affiliated with multi-bank holding companies are also excluded. These exclusions reduce the sample to 470,118 bank-quarters.

I exclude all bank-quarters in which a merger occurs because these confound balance sheet measures of changes in liabilities and lending. Observations in the first three years of a bank's operations are also excluded because these banks face a different sort of regulatory supervision (Singh 2007). Banks with non-positive total assets and those missing audit indicators or required financial data are all removed. Finally, I exclude observations with quarterly total asset growth greater than 50 percent to minimize the influence of potential data errors and outliers. The final sample in Table 1 includes 422,602 bank-quarters.

Descriptive Statistics

Table 2, Panel A reports summary statistics partitioning the sample by audit status. Audited banks, which represent 55 percent of the sample, are generally larger, so I perform additional analyses in Section V to ensure that bank size does not confound my results. In general, audited banks have fewer liquid assets and invest more in loans, including illiquid loans such as commercial and industrial (C&I) loans.[15] On the liability side, audited banks hold less equity and rely less on core deposits, which are deposit accounts with balances of $100,000 or below, as a funding source. However, they have a higher level of managed liabilities such as large CDs. Untabulated univariate correlations indicate a positive correlation between audits and the use of managed liabilities (correlation = 0.19). In addition, audits are positively correlated with bank size (correlation = 0.36) and location in urban areas (correlation = 0.19). However, they are negatively associated with the level of liquid assets (correlation = −0.11).

Table 2, Panel B presents summary statistics partitioning the sample into tightening and non-tightening periods. Consistent with tight money and the predicted effect of audits, managed liabilities grow more rapidly in tightening periods than in other periods, particularly for audited banks (p-value for the difference-in-differences in mean = 0.001). However, because univariate comparisons do not control for bank differences associated with audits or concurrent macroeconomic changes, I use regression analyses to test my hypotheses.

Testing the Financing and Investment Benefits of Audited Accounting Information

Table 3 presents the H1 financing test results, while Table 4 shows the H2 loan test results. In Regressions (1) and (2) of both tables, the monetary contraction variable (*TightMP*) includes policy shocks only in the contemporaneous quarter, thus assuming no impact of current contractionary actions on subsequent quarters. In Regressions (3) and (4), I add five lagged policy terms to capture delays in banks' responses to monetary tightening. The coefficients associated with *TightMP* in these two columns are the sums of the six coefficients on the contemporaneous and the lagged monetary policy variables. For ease of interpretation, I mean-adjust the bank-level control variables so that the main effect of *TightMP,* as captured by βs, can be interpreted as the change triggered by monetary tightening for an unaudited bank with average bank characteristics.[16] The t-statistics in parentheses, both for individual coefficients and the sums of coefficients on policy terms, are computed using robust standard errors clustered by bank and quarter. In columns (3) and (4), I also report p-values for the F-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly 0. For brevity, the tables omit economy-wide and other factors discussed in Section III.

Statistical Significance

The dependent variable in Table 3 is quarterly change in managed liabilities (*ML_Change*). Results reported in column (1) show that the coefficient on *TightMP* is significantly positive, consistent with small banks issuing more managed liabilities in response to contractionary actions. More importantly, in line with H1 that audited banks face fewer funding frictions, the coefficient on *TightMP* × *Audited* is significantly positive (t = 13.54).

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[15] Audited banks may tend to lend more and, hence, experience higher loan growth during tightening periods. To address this concern, I repeat the main tests after adding beginning-of-period loan-to-assets ratio as an additional control and find that the inference remains unaffected.

[16] Specifically, I adjust each bank-level control by subtracting its sample mean and then scaling the difference by its standard deviation. Because the adjusted controls are mean zero, the main effect of *TightMP* captures the change due to monetary tightening for an unaudited bank with average values for bank controls.

TABLE 2

Descriptive Statistics

Panel A: Sample by Audit Status

	Full Sample		Audited Sample		Unaudited Sample	
	Mean	**Median**	**Mean**	**Median**	**Mean**	**Median**
Total assets (millions)	79.15	53.62	101.37	73.84	52.04**	37.12**
Log of total assets	10.89	10.89	11.18	11.21	10.54**	10.52**
Liquid assets (÷ by total assets)	0.347	0.332	0.332	0.316	0.365**	0.352**
Total loans (÷ by total assets)	0.546	0.560	0.560	0.574	0.530**	0.542**
C&I loans (÷ by total loans)	0.165	0.141	0.174	0.147	0.154**	0.134**
Real estate loans (÷ by total loans)	0.540	0.550	0.582	0.597	0.490**	0.496**
Nonperforming loans (÷ by total loans)	0.015	0.009	0.016	0.009	0.015*	0.009*
Total Liabilities (÷ by total assets)	0.899	0.907	0.903	0.909	0.895**	0.903**
Core deposits (÷ by total liabilities)	0.725	0.745	0.698	0.719	0.757**	0.774**
Managed liabilities (÷ by total liabilities)	0.231	0.211	0.255	0.235	0.202**	0.184**
Large CDs (÷ by total liabilities)	0.117	0.102	0.127	0.112	0.104**	0.090**
Equity (÷ by total assets)	0.101	0.093	0.097	0.091	0.105**	0.097**
Other Variables used in the Main Tests						
Quarterly change in managed liabilities (*ML_Change*)	−0.001	0.001	−0.002	0.002	−0.001**	0.000**
Quarterly change in total loans (*Loan_Change*)	0.027	0.024	0.028	0.025	0.026**	0.022**
Controlled by a one-bank holding company (*OBHC*)	0.665	1.000	0.648	1.000	0.684**	1.000**
Located in a metropolitan statistical area (*MSA*)	0.308	0.000	0.389	0.000	0.210**	0.000**
Low equity ratio indicator (*LowCap*)	0.037	0.000	0.045	0.000	0.027**	0.000**
Loss indicator (*Loss*)	0.068	0.000	0.073	0.000	0.062**	0.000**
Internal cash flows (*Internal CF*)	0.006	0.006	0.006	0.006	0.007**	0.006*
Number of observations	422,602		232,241		190,361	
% of Full Sample			(55%)		(45%)	

(continued on next page)

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TABLE 2 (continued)

Panel B: Sample by Periods

	Audited Sample				Unaudited Sample				Diff-in-Diff in Means [i.e., (A-B) – (A1-B1)]
	(A) Tightening Periods		(B) Other Periods		(A1) Tightening Periods		(B1) Other Periods		
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	p-values
Total assets (millions)	99.08	73.34	102.94##	74.15##	50.94	36.46	52.84##	37.60##	0.003
Log of total assets	11.16	11.20	11.20##	11.21##	10.50	10.50	10.57##	10.53##	0.001
Liquid assets (÷ by total assets)	0.324	0.307	0.337##	0.321##	0.356	0.342	0.371##	0.359##	0.042
Total loans (÷ by total assets)	0.565	0.580	0.556##	0.571##	0.538	0.551	0.525##	0.536##	0.001
C&I loans (÷ by total loans)	0.174	0.147	0.174	0.147	0.154	0.135	0.153	0.134	0.300
Real estate loans (÷ by total loans)	0.581	0.595	0.582	0.598	0.489	0.495	0.491	0.497	0.394
Nonperforming loans (÷ by total loans)	0.015	0.008	0.016	0.010#	0.015	0.008	0.015	0.009#	0.001
Total Liabilities (÷ by total assets)	0.902	0.909	0.903#	0.910#	0.894	0.903	0.895#	0.903	0.894
Core deposits (÷ by total liabilities)	0.681	0.700	0.709##	0.730##	0.742	0.758	0.767##	0.785##	0.203
Managed liabilities (÷ by total liabilities)	0.258	0.238	0.253##	0.233##	0.204	0.187	0.200##	0.183##	0.050
Large CDs (÷ by total liabilities)	0.132	0.115	0.124##	0.109##	0.107	0.093	0.102##	0.088##	0.001
Equity (÷ by total assets)	0.098	0.091	0.097#	0.090#	0.106	0.097	0.105#	0.097#	0.888
Other Variables used in the Main Tests									
Quarterly change in managed liabilities (*ML_Change*)	0.017	0.012	−0.014##	−0.010##	0.011	0.003	−0.010##	−0.009##	0.001
Quarterly change in total loans (*Loan_Change*)	0.024	0.022	0.031##	0.027##	0.018	0.019	0.032##	0.027##	0.001
Controlled by a one-bank holding company (*OBHC*)	0.652	1.000	0.646#	1.000	0.682	1.000	0.686	1.000	0.114
Located in a metropolitan statistical area (*MSA*)	0.354	0.000	0.412##	0.000##	0.198	0.000	0.220##	0.000##	0.001
Low equity ratio indicator (*LowCap*)	0.045	0.000	0.046	0.000	0.030	0.000	0.024	0.000	0.013
Loss indicator (*Loss*)	0.072	0.000	0.074	0.000	0.070	0.000	0.056#	0.000#	0.001
Internal cash flows (*Internal CF*)	0.006	0.006	0.006	0.006	0.007	0.006	0.007	0.006	0.339
Number of observations	94,250		137,991		80,329		110,032		

**, * In Panel A, denote a statistically significant difference between audited and unaudited banks at the 1 percent and 5 percent level, respectively.
##, # In Panel B, denote a statistically significant difference between tightening and other periods for audited banks (columns (A)–(B)) or for unaudited banks (columns (A1)–(B1)) at the 1 percent and 5 percent level, respectively.
This table reports common balance sheet and other selected information about the sample. The sample and data are collected from the Call Reports, including 10,587 banks and a total number of 422,602 bank-quarters in the period from 1988: Q1–2007: Q2.
Variable definitions are presented in Appendix A.

TABLE 3

H1: Comparing the Responses of Managed Liabilities to Monetary Tightening across Audited and Unaudited Banks

	Pred. Sign	No Lagged Policy Variables (1) Coeff.	(1) t-stat.	No Lagged Policy Variables (2) Coeff.	(2) t-stat.	Adding Five Lagged Policy Variables[a] (3) Coeff.	(3) t-stat.	Adding Five Lagged Policy Variables[a] (4) Coeff.	(4) t-stat.
Intercept		−0.0451	(−1.30)	−0.0449	(−1.32)	−0.0356	(−1.00)	−0.0368	(−1.07)
Monetary Policy Variables									
TightMP	+	0.0146	(3.58)	0.0191	(4.85)	0.0228	(2.59)	0.0319	(4.22)
TightMP × *Audited*	+	0.0112	(13.54)	0.0037	(6.31)	0.0212	(13.62)	0.0069	(4.99)
TightMP × *Ln(TA)*	+			0.0057	(2.74)			0.0118	(6.90)
TightMP × *OBHC*	?			0.0015	(2.84)			0.0035	(4.89)
TightMP × *MSA*	+			0.0047	(6.45)			0.0113	(2.94)
TightMP × *LowCap*	−			−0.0012	(−4.24)			−0.0010	(−4.18)
TightMP × *Loss*	−			−0.0011	(−3.67)			−0.0029	(−4.19)
TightMP × *NPL*	−			−0.0023	(−2.12)			−0.0030	(−2.44)
TightMP × *Liquid Assets*	−			−0.0087	(−7.39)			−0.0111	(−4.48)
TightMP × *Internal CF*	−			−0.0032	(−3.40)			−0.0049	(−5.62)
Bank-Level Variables									
Audited		−0.0023	(−3.07)	0.0009	(1.55)	−0.0063	(−4.25)	−0.0002	(−0.31)
Ln(TA)		−0.0066	(−10.49)	−0.0089	(−8.79)	−0.0066	(−10.15)	−0.0115	(−10.97)
OBHC		0.0006	(4.48)	0.0001	(0.40)	0.0006	(4.13)	−0.0006	(−3.95)
MSA		−0.0001	(−0.16)	−0.0018	(−2.25)	−0.0002	(−0.47)	−0.0046	(−2.77)
LowCap		−0.0027	(−6.65)	−0.0020	(−3.84)	−0.0027	(−6.60)	−0.0022	(−4.07)
Loss		−0.0018	(−2.57)	−0.0014	(−1.87)	−0.0018	(−2.68)	−0.0011	(−1.66)
NPL		0.0022	(3.10)	0.0031	(4.65)	0.0021	(3.05)	0.0033	(6.05)
Liquid Assets		0.0049	(1.85)	0.0085	(2.89)	0.0050	(1.88)	0.0097	(3.19)
Internal CF		0.0009	(0.93)	0.0021	(1.68)	0.0009	(0.98)	0.0029	(3.55)
Economy-wide and other factors		Yes		Yes		Yes		Yes	
Number of observations		422,602		422,602		422,602		422,602	
Adj. R^2		0.2017		0.2040		0.2132		0.2178	

(continued on next page)

TABLE 3 (continued)

	Pred. Sign	No Lagged Policy Variables (1) Coeff.	(1) t-stat.	No Lagged Policy Variables (2) Coeff.	(2) t-stat.	Adding Five Lagged Policy Variables[a] (3) Coeff.	(3) t-stat.	Adding Five Lagged Policy Variables[a] (4) Coeff.	(4) t-stat.
F-stat. and p-value for the joint significance of the five lagged *TightMP* × *Audited*						43.66 (0.001)		16.05 (0.001)	

[a] Sums of coefficients are shown for the policy variables.

This table reports results from regressing the quarterly change in managed liabilities on the contractionary policy indicators (*TightMP*), an audit indicator for the bank's audit status in the previous year (*Audited*), the policy-audit interaction terms (*TightMP* × *Audited*), and various control variables. Regressions (1) and (2) include only policy shocks in the contemporaneous quarter ($TightMP_t$), assuming no delays in banks' responses to monetary tightening. In Regressions (3) and (4), I add five lagged terms of *TightMP* to capture delays in banks' responses to monetary tightening. The coefficients on the *TightMP*-related variables in these two columns are the sums of the six coefficients on the contemporaneous and the lagged monetary policy variables. The t-statistics in parentheses are based on robust standard errors clustered by bank and quarter. In columns (3) and (4), I also report p-values for the F-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly 0.

The specification for Regression (2) is shown below:

$$D\_Change_{it} = \alpha + \beta TightMP_t + \gamma Audited_{it-4} + \delta TightMP_t \times Audited_{it-4} + \zeta Bank\text{-}Level\ Control_{it-1} + \eta TightMP_t \times Bank\text{-}Level\ Control_{it-1} + \ldots + \sum_{j=1}^{5} \theta_j D\_Change_{it-j}$$
$$+ \sum_{j=0}^{5} \iota_j GDP\_Growth_{t-j} + \sum_{j=0}^{5} \kappa_j CPI\_Growth_{t-j} + \lambda Basel_t + \sum_{j-1}^{50} \nu_j State_{ij} + \xi Time\ Trend_t + \sum_{j=1}^{3} o_j Quarter_{jt} + \varepsilon_{it},$$

where *D_Change* captures the quarterly change in managed liabilities, and *TightMP* is an indicator that equals 1 if contractionary policies take place in the quarter. Bank-level controls include the natural log of total assets (*Ln*(*TA*)), a set of indicators for banks owned by a one-bank holding company (*OBHC*), banks located in a metropolitan statistical area (*MSA*), banks with an equity-to-assets ratio below 6 percent (*LowCap*), loss-making banks (*Loss*), the ratio of nonperforming loans to total loans (*NPL*), the ratio of liquid assets to total assets (*Liquid Assets*), and the ratio of internally generated cash flows to total loans (*Internal CF*). Each regression also includes five lags of the dependent variable (*D_Change*), current and five lags of each of the growth rates of GDP (*GDP_Growth*) and the consumer price index (*CPI_Growth*), an indicator for the period 1988–1992 (*Basel*), a set of state indicators, a time trend, and a set of three quarter indicators. See Appendix A for variable definitions. To facilitate interpretation of results, the control variables are mean-adjusted so that the coefficients on the main effect of *TightMP* (β) can be interpreted as the changes triggered by monetary tightening for an unaudited bank with average bank characteristics. The sample consists of 422,602 bank-quarters in the period from 1988: Q1–2007: Q2.

In Regressions (3) and (4), I add five lagged terms of *TightMP* to capture delays in banks' responses to monetary tightening, as shown in Figure 2. The specification for Regression (4) is shown below:

$$D\_Change_{it} = \alpha + \sum_{j=0}^{5} \beta_j TightMP_{t-j} + \gamma Audited_{it-4} + \sum_{j=0}^{5} \delta_j TightMP_{t-j} \times Audited_{it-4} + \zeta Bank\text{-}Level\ Control_{it-1} + \sum_{j=0}^{5} \eta_j TightMP_{t-j} \times Bank\text{-}Level\ control_{it-1} + \ldots$$
$$+ \sum_{j=1}^{5} \theta_j D\_Change_{it-j} + \sum_{j=0}^{5} \iota_j GDP\_Growth_{t-j} + \sum_{j=0}^{5} \kappa_j CPI\_Growth_{t-j} + \lambda Basel_t + \sum_{j=1}^{50} \nu_j State_{ij} + \xi Time\ Trend_t + \sum_{j=1}^{3} o_j Quarter_{jt} + \varepsilon_{it}.$$

TABLE 4

H2: Comparing the Responses of Total Loans to Monetary Tightening across Audited and Unaudited Banks

	Pred. Sign	No Lagged Policy Variables (1) Coeff.	(1) t-stat.	No Lagged Policy Variables (2) Coeff.	(2) t-stat.	Adding Five Lagged Policy Variables[a] (3) Coeff.	(3) t-stat.	Adding Five Lagged Policy Variables[a] (4) Coeff.	(4) t-stat.
Intercept		0.0232	(2.65)	0.0243	(2.23)	0.0201	(1.92)	0.0191	(1.67)
Monetary Policy Variables									
TightMP	−	−0.0171	(−8.96)	−0.0156	(−8.14)	−0.0268	(−8.75)	−0.0264	(−8.87)
TightMP × *Audited*	+	0.0069	(4.86)	0.0040	(5.41)	0.0079	(6.12)	0.0063	(6.59)
TightMP × *Ln(TA)*	+			0.0081	(27.02)			0.0091	(16.47)
TightMP × *OBHC*	?			0.0004	(1.35)			0.0005	(1.66)
TightMP × *MSA*	+			0.0012	(2.11)			0.0058	(7.03)
TightMP × *LowCap*	−			−0.0002	(−0.94)			−0.0007	(−1.84)
TightMP × *Loss*	−			−0.0017	(−5.49)			−0.0016	(−5.61)
TightMP × *NPL*	−			−0.0001	(−0.13)			−0.0004	(−0.83)
TightMP × *Liquid Assets*	+			0.0131	(28.33)			0.0155	(25.89)
TightMP × *Internal CF*	+			0.0016	(2.98)			0.0027	(3.11)
Bank-Level Variables									
Audited		−0.0030	(−1.45)	−0.0005	(−0.44)	−0.0037	(−1.80)	−0.0021	(−1.15)
Ln(TA)		−0.0007	(−0.49)	−0.0040	(−2.18)	−0.0007	(−0.40)	−0.0044	(−2.36)
OBHC		−0.0002	(−0.88)	−0.0006	(−1.56)	−0.0003	(−1.14)	−0.0005	(−1.64)
MSA		0.0012	(1.21)	0.0006	(0.72)	0.0012	(1.19)	0.0003	(0.55)
LowCap		−0.0014	(−5.31)	−0.0013	(−4.24)	−0.0013	(−6.41)	−0.0010	(−3.98)
Loss		−0.0016	(−3.10)	−0.0010	(−3.29)	−0.0017	(−3.67)	−0.0012	(−3.97)
NPL		0.0056	(9.10)	0.0057	(8.17)	0.0056	(9.79)	0.0058	(9.16)
Liquid Assets		−0.0059	(−3.51)	−0.0113	(−10.54)	−0.0058	(−3.63)	−0.0143	(−12.33)
Internal CF		0.0008	(1.21)	0.0001	(0.04)	0.0008	(1.14)	−0.0004	(−0.50)
Economy-wide and other factors		Yes		Yes		Yes		Yes	
Number of observations		422,602		422,602		422,602		422,602	
Adj. R^2		0.1092		0.1235		0.1218		0.1305	

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TABLE 4 (continued)

	Pred. Sign	No Lagged Policy Variables (1) Coeff.	(1) t-stat.	No Lagged Policy Variables (2) Coeff.	(2) t-stat.	Adding Five Lagged Policy Variables[a] (3) Coeff.	(3) t-stat.	Adding Five Lagged Policy Variables[a] (4) Coeff.	(4) t-stat.
F-stat. and p-value for the joint significance of the five lagged *TightMP* × *Audited*						20.95 (0.001)		38.55 (0.001)	

[a] Sums of coefficients are shown for the policy variables.

This table reports results from regressing the quarterly change in total loans on the contractionary policy indicators (*TightMP*), an audit indicator for the audit status in the previous year (*Audited*), the policy-audit interaction terms (*TightMP* × *Audited*), and various control variables. See Table 3 for model specification. The t-statistics in parentheses are based on robust standard errors clustered by bank and quarter. In columns (3) and (4), I also report p-values for the F-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly 0.

The results supporting H1 are robust to controlling for factors related to bank size, operating environments, bank risk-taking, and liquidity position. Consistent with prior studies, Table 3, column (2) shows that larger banks and banks located in urban areas (*MSA*) have greater access to managed liabilities when reacting to tight policies. At the same time, such access is generally negatively correlated with attributes related to bank risk, such as a low level of equity-to-assets ratio, frequency of losses, nonperforming loans, and availability of internal funds, as reflected in the holding of liquid assets and the ability to generate internal cash flows. Columns (3) and (4) report the results that include the contemporaneous and five lags of monetary policy. The coefficients on *TightMP*-associated variables become generally larger, consistent with delays in banks' responses to monetary tightening.

Turning to the test of H2, Table 4 shows the results of the regressions using quarterly change in total loans as the dependent variable (*Loan_Change*). Column (3) shows that audited banks can better maintain their lending levels following Fed-induced liquidity losses; the sum of the coefficients on *TightMP* × *Audited* is positive ($t = 6.12$). As shown in column (4), the results are robust to adding controls for different bank-level characteristics. In line with prior studies, the lending responses are positively associated with bank size, an urban location, and internal funds. On the other hand, they are largely negatively correlated with bank risk.

The main effects of bank-level variables measure the associations between these variables and the dependent variable during non-tightening periods. Consistent with slower growth for larger banks, column (4) of Tables 3 and 4 shows that the coefficient on bank size is negative and significant. Banks with more binding capital constraints also seem to have slower growth. There is no evidence of differential growth for audited banks.

Economic Significance

To interpret the economic benefits for audited banks, I focus on the results that include policy lags, as reported in column (4) of Tables 3 and 4. First, I use the results to predict an average bank's responses to a given contractionary quarter, including the effects in quarters Q to Q+5 in Figure 2, Panel A. Based on the sum of the coefficients on *TightMP*, an unaudited bank responding to monetary contraction in quarter Q is estimated to have a cumulative 3.19 percent increase in managed liabilities relative to loans over the subsequent five quarters. This translates into about \$1.38 million of managed liabilities, given that the average bank has \$43.22 million of loans (= 79.15 × 0.546 from Table 2, Panel A). Importantly, the sum of the coefficients on *TightMP* × *Audited* suggests that an external audit is associated with an additional 0.69 percent increase, which is about \$0.30 million of managed liabilities. Turning to loan growth, the model predicts a cumulative 2.64 percent decline in loans for an unaudited bank five quarters after the given contractionary quarter, which equals \$1.14 million of loans.[17] An external audit is expected to mitigate such a decline by 23.86 percent (= 0.63/2.64) to about \$0.87 million of loans, suggesting that audits can weaken the effect of monetary contractions on small bank lending.

Alternatively, we can use the results to estimate bank changes in a quarter after multiple contractionary actions in prior periods. Specifically, consider the final quarter of a six-quarter contractionary cycle, shown as quarter Q in Figure 2, Panel B. The sum of the coefficients on *TightMP* × *Audited* suggests that the increase in managed liabilities relative to loans in quarter Q is 0.69 percentage points higher for an audited bank than for an otherwise similar unaudited bank. Correspondingly, the decline in loans in the quarter is predicted to be 0.63 percentage points lower for the audited bank. These are economically relevant effects in this context, considering that the

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[17] This is economically meaningful; aggregating across all unique banks in the sample, this suggests, roughly, loan declines of \$12.07 billion (= \$1.14 million × 10,587 banks) due to one contractionary quarter. There are usually multiple contractionary quarters in a given contractionary cycle.

unconditional mean (median) quarterly change in managed liabilities for the sample period is only −0.1 (0.1) percent and that for loans is 2.7 (2.4) percent, as shown in Table 2, Panel A.

V. FURTHER TESTS

Equation (1) provides the basis for the tests in this section. For ease of presentation, except when otherwise stated, only the sum of the coefficients on *TightMP* ($\Sigma\beta$) and that on *TightMP* × *Audited* ($\Sigma\delta$) are reported. Table 5, Panel A presents the H1 financing test results, while Panel B shows the H2 loan test results. Column (1) shows the benchmark results from column (4) of Tables 3 and 4.

Addressing Potential Self-Selection Bias

The Heckman Two-Step Approach

I first apply the Heckman (1979) two-stage approach as an alternate way to address potential selection bias, discussed in Section III. In the first-stage selection model, I include an indicator variable for the presence of an audit five years ago (*PastAudit*) to meet the exclusion restriction requirement. This is motivated by the sticky nature of an audit (Kohlbeck 2005). A partial explanation for this stickiness is that bank supervisors require banks to provide full explanations when they terminate an external auditor (*Federal Register* 1999). To avoid regulatory complications, small banks with audits in the past are likely to be associated with the same audit status in the future. However, due to the significant time lags, it is unlikely that a bank's past audit choice would directly affect current changes in managed liabilities or total loans, the dependent variable in the second-stage regression, that happen more than five years later.

In addition to *PastAudit,* I follow prior research and include a number of other variables in the probit model of audit choice. For brevity, I relegate the details of these variables to Appendices B and C. The model is estimated using annual data. Appendices B and C report the results for models with and without the inclusion of *PastAudit*. While both models predict the audit choice reasonably well, the one with *PastAudit* fits the data particularly well, having the ability to sort banks into the right group more than 90 percent of the time. As expected, there is a strong association between *PastAudit* and current audit status. Therefore, I use the estimates to compute the inverse Mills ratio (*IMR*) for each sample bank. In the second-stage policy response regressions, *IMR* and its interaction terms with monetary policy serve as additional control variables. Column (2) shows that the positive effect associated with audits remains. Further, the sum of the coefficients on *TightMP* × *IMR* indicates that the main results are negatively biased, possibly because banks inherently prone to liquidity issues endogenously get audited as a precaution.[18]

Predicted Probability of Having an Audit as an Instrument

While my audit choice model has reasonably strong prediction power, misspecification of the model can still affect the Heckman test results. As such, I use an additional three-step approach suggested in Chang et al. (2009) to ensure the robustness of the results. This approach involves using the predicted probability of having an audit as an instrument in instrumental regressions.[19]

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[18] While including *PastAudit* in the prediction model leads to higher prediction power, there is concern that the regression is somewhat circular in that it may fail to adequately control for sticky firm factors that determine audit choice in the preceding year. As a robustness check, I repeat the tests using estimates from the prediction model without *PastAudit* and confirm that the results continue to hold.

[19] First, I predict the probability of a small bank having an audit using the previous prediction model. In the next two steps, I use the predicted probability as an instrument and apply a standard instrumental regression to evaluate the effect of audits. Consistent with Chang et al. (2009), the interaction term between *Audited* and *TightMP* is based on the demeaned value of *TightMP*.

TABLE 5

Further Tests

Panel A: *ML_Change*

	Benchmark Results (1)		Heckman Two-Stage Test (2)		Instrumental Regressions (3)		Audit-Switched Banks (4)		Bank-Fixed Effects (5)		Below-Median Liquid Assets (6)		Above-Median Liquid Assets (7)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
TightMP	0.0319	(4.22)	0.0377	(11.09)	0.0397	(13.97)	0.0308	(3.60)	0.0288	(3.32)	0.0529	(7.10)	0.0093	(0.87)
TightMP × *Audited*	0.0069	(4.99)	0.0098	(2.20)	0.0063	(2.62)	0.0059	(2.36)	0.0060	(4.85)	0.0110	(5.17)	0.0026	(1.32)
TightMP × *IMR*			−0.0023	(−1.77)										
Number of observations	422,602		271,228		271,228		178,071		244,531		211,377		211,225	
Adj. R^2		0.2178		0.1437		0.1437		0.1333		0.2971		0.1782		0.2375
Chi-square test and p-value for difference in coeff. of *TightMP* × *Audited* between Regressions (6) and (7)											13.17 (0.001)			

Panel B: *Loan_Change*

	Benchmark Results (1)		Heckman Two-Stage Test (2)		Instrumental Regressions (3)		Audit-Switched Banks (4)		Bank-Fixed Effects (5)		Below-Median Liquid Assets (6)		Above-Median Liquid Assets (7)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
TightMP	−0.0264	(−8.87)	−0.0243	(−6.71)	−0.0258	(−8.34)	−0.0269	(−7.80)	−0.0225	(−10.16)	−0.0394	(−7.94)	−0.0038	(−1.65)
TightMP × *Audited*	0.0063	(6.59)	0.0076	(3.74)	0.0067	(8.57)	0.0062	(5.98)	0.0055	(6.52)	0.0138	(6.96)	0.0012	(1.22)
TightMP × *IMR*			−0.0015	(−1.81)										
Number of observations	422,602		271,228		271,228		178,071		244,531		211,377		211,225	
Adj. R^2		0.1305		0.1761		0.1760		0.1798		0.1930		0.2045		0.1718
Chi-square test and p-value for difference in coeff. of *TightMP* × *Audited* between Regressions (6) and (7)											13.78 (0.001)			

(continued on next page)

TABLE 5 (continued)

This table presents results on further tests. For ease of exposition, except for column (2), only the sum of the coefficients on monetary tightening (*TightMP*) ($\Sigma\beta$) and that on the interaction term *TightMP* × *Audited* ($\Sigma\delta$) are reported. Panel A presents the results for the quarterly change in managed liabilities (*ML_Change*), while Panel B shows the loan regression results (*Loan_Change*). Regression (1) is the benchmark regression, as shown in column (4) of Tables 3 and 4. See Table 3 for baseline model specifications. The t-statistics in parentheses are computed using robust standard errors clustered by bank and quarter.

Addressing potential self-selection bias: Regression (2) is the second -stage regression of the Heckman test. Regression (3) uses the predicted probability of an audit from the probit model in Appendix B as an instrument. Regression (4) studies only the group of banks that change audit status during the sample period. Regression (5) is a bank fixed effects regression, including only the group of banks that have the same audit status throughout the 20-year sample period.

Cross-sectional analyses based on the holding of liquid assets: Regressions (6) and (7) include subsamples of banks with different levels of liquid assets.

Wooldridge (2002) shows that this approach mitigates the effect of a misspecified choice model on the estimation of the treatment effects. Table 5, column (3) shows that the main results continue to hold under this approach.

Within- and Across-Bank Variations in Audit Status

Given the difficulty of finding truly exogenous variables that satisfy the requirement of exclusion restrictions, prior studies have questioned the efficacy of using two-stage approaches over single-stage methods (Lennox, Francis, and Wang 2012). To ensure that my results are robust to alternate model specifications, I study the subsample of banks that change audit status and repeat the tests using each bank as its own control. As expected, I find that banks can better withstand liquidity losses when they are audited than when they are not, as shown in Table 5, column (4).

On the other hand, changes in audit status might reflect changes in other bank operations that can directly affect banks' responses to monetary tightening. For example, changes in managers might lead to changes in both a bank's audit choice, as well as how it reacts to monetary policy. To minimize such confounding changes, I also compare policy responses among banks that have the *same* audit status throughout the 20-year sample period. For these banks, some external factors, such as managers' past experience working with CPAs, may have consistently affected their audit decisions. While it is unclear whether these factors would directly affect how banks react to monetary actions, to the extent that they do, bank fixed effects regressions would address their impact. Column (5) of Table 5 shows that the main results persist within this subsample even after I control for bank fixed effects.

Alternative Explanations and Other Robustness Checks

Differential Impacts of Tight Money on Lending Opportunities

Smaller, less sophisticated banks have different clienteles, including concentrations of loans to small businesses and farms. Prior research suggests that smaller, less sophisticated businesses are more susceptible to shifts in economic conditions because they lack the financial flexibility of larger, professionally managed firms (Bernanke, Gertler, and Gilchrist 1996). If monetary tightening affects small businesses more than larger ones, then smaller, less sophisticated lenders will be more affected by Fed policy. Thus, unaudited banks' lending opportunities may be more restricted during periods of monetary tightening because their clients are less able to invest. To address this issue, I add a proxy for concentrations of small business loans and its interaction terms with *TightMP* as controls. From June 1993 onward, the Call Report database provides the total amount of small loans, defined as loans originated for less than $100,000, for each of these four loan categories: (1) commercial and industrial loans, (2) real estate loans secured by nonfarm nonresidential properties, (3) loans used to finance agricultural production, and (4) real estate loans secured by farmland. Assuming that these loans are predominately made to small businesses, I define small business loan concentration as the ratio of the sum of these loans to total loans. Bank regulators and prior studies (Ashcraft 2006) make similar assumptions. Unreported tests show that the effect of audits persists after adding these controls.

Differential Concerns about Credit Quality and Loan Sales

Banks concerned about their clients' ability to cope with an environment of increasing interest rates can sell loans to limit their credit exposures. Unaudited banks may be more concerned about declining credit quality because small businesses are more susceptible to shifts in economic conditions. Hence, unaudited banks may sell more loans during tight periods, which could confound my results. However, this issue is partially mitigated because my results are robust to

controlling for differences in clienteles between audited and unaudited banks. To further address this issue, I adjust the loan growth measure by adding back loans sold. Note that these adjustments are likely to weaken my results because this will remove the effect of loan sales for the purpose of raising liquidity. The data for sold loans are only available for periods before 1994. Despite the limited sample size, unreported tests show that the effect of audits remains significant at the 10 percent level, consistent with audited banks being better able to maintain their lending levels.

The Effect of Growth on Audit Choice

Because audits become mandatory when the bank's total assets exceed $500 million, growth could lead banks to obtain audits if they anticipate crossing the threshold. Thus, audited banks may tend to have more growth opportunities, which might explain why they show stronger growth during monetary tightening. This issue is more likely to affect banks with total assets close to the $500 million threshold. However, because such banks only constitute a small proportion of my sample, with only about 2.5 percent of the banks having total assets between $300 million and $500 million, this type of endogeneity is unlikely to confound my results. Unreported tests show that the main results persist if I exclude banks with total assets more than $300 million. Note, too, that the main tests already adjust for growth trends by including five lags of the dependent variable as controls.

Analyses within Similarly Sized Banks

I repeat the tests separately for subsamples of similarly sized banks to ensure the main results are not driven by size. The test divides the full sample into five size groups that are often applied by investors and regulators. Banks in the smallest group with total assets below $25 million are excluded to ensure that the banks have sufficient ability to obtain managed liabilities. To reduce the effect of banks anticipating crossing the size threshold, as discussed before, the largest banks with total assets between $300 million and $500 million are also excluded. Unreported tests show that the impact of audits persist in each of the three remaining size partitions, (A) $25 million–$50 million; (B) $50 million–$100 million; and (C) $100 million–$300 million, suggesting that difference in size between audited and unaudited banks is unlikely to confound my results.

Cross-Sectional Analyses Based on the Holding of Liquid Assets

I exploit the cross-sectional variation in liquid assets to further demonstrate the effect of audits. As discussed before, the predicted audit effect is expected to arise mainly among banks with fewer liquid assets. I first partition the sample into high- or low-liquid asset banks based on the sample median for each quarter. I then repeat the tests separately for the two subsamples. Differences between audited and unaudited banks' sensitivity to monetary tightening are tested using a Chi-square statistic. As expected, columns (6) and (7) of Table 5 suggest that the effect of audits is limited to banks with fewer beginning-of-period liquid assets, indicating the greater importance of credible reporting when the bank lacks liquid assets to sell and, hence, has a greater need to enter the uninsured financing market. In contrast, banks with more liquid assets are largely insensitive to the liquidity shocks created by monetary tightening, consistent with the findings in prior studies (Kashyap and Stein 2000).

Path Analysis of the Direct and Indirect Effects of Audits

I hypothesize that audits can affect loan growth during tightening periods both directly through expectation about future liquidity constraints, and indirectly through growth in managed liabilities. Thus, the main tests focus on the combined effect of audits. In this section, I use a path analysis to

TABLE 6

Path Analysis of Direct and Indirect Effects of External Audits on Loan Growth

	Coeff.	t-stat
Direct Path		
p(Audit, Loan growth)	0.0015	(1.87)
Indirect Path		
a. *p*(Audit, Growth in managed liabilities)	0.0044	(8.98)
b. *p*(Growth in managed liabilities, Loan growth)	0.4902	(15.32)
Total magnitude of indirect path (a × b)	0.0022	(9.62)
Bank-level controls	Yes	
Economy-wide controls	Yes	
Number of observations	174,579	
Goodness of fit index	0.9893	
Adjusted goodness of fit index	0.9772	

This table reports results from a path analysis that examines the direct effect of external audits on loan growth and the indirect effect through growth in managed liabilities. p(X1,X2) stands for unstandardized path coefficient. A recursive path model with observable variables is used, and the analysis is based on observations from tightening periods.

illuminate the relative importance of the direct versus indirect effects. This analysis can also reveal whether the maintained assumption that access to managed liabilities affects loan growth holds in my sample.

The path analysis decomposes the correlation between a source variable and an outcome variable into (1) a direct path between these two variables, and (2) an indirect path through a mediating variable. The source, outcome, and mediating variables in this study refer to external audits, loan growth, and concurrent growth in managed liabilities, respectively. Following prior research (Landsman, Maydew, and Thornock 2012), I use a recursive path model with observable variables. I estimate the following structural equation model using observations from tightening periods:

$$Loan\_Change_{it} = \alpha_1 + \alpha_2 Audited_{it-4} + \alpha_3 ML\_Change_{it} + \alpha_4 Controls + \varepsilon_{it}. \tag{2}$$

$$ML\_Change_{it} = \beta_1 + \beta_2 Audited_{it-4} + \varepsilon_{it}. \tag{3}$$

The path coefficient α_2 is the magnitude of the direct path from audits to loan growth, whereas the product of the path coefficients $\alpha_3 \times \beta_2$ is the magnitude of the indirect path from audits to loan growth through managed liabilities. Table 6 reports the path coefficients of interest. The path coefficient between audits and managed liabilities (β_2) is positive and significant, consistent with results in Table 3 that audited banks have greater access to managed liabilities. In addition, consistent with prior studies that suggest that access to managed liabilities increases a bank's ability to lend during tightening periods (Peek and Rosengren 2013), the path coefficient between growth in managed liabilities and loan growth (α_3) is positive and significant. As expected, managed liabilities act as an important mediating variable: the total magnitude of the indirect path is about 60 percent of the combined effect of audits on loan growth ($= 0.0022/(0.0015 + 0.0022)$). The results also indicate a significant direct effect of audits; the path coefficient between audits and loan growth (α_2) is positive and significant, consistent with audited banks having fewer concerns about future liquidity constraints and a greater willingness to lend even in a tight money cycle.

VI. CONCLUSION

I provide evidence that small, non-public banks with audited financial statements are less affected by liquidity shortages created by monetary tightening than unaudited banks. The results suggest that audited banks have greater access to uninsured financing to counteract Fed-induced liquidity shortages. Accordingly, audited banks seem better able to maintain their investments in the form of lending during periods of tight money. These results are relevant for understanding how external audits and reporting credibility affect financial intermediation by banks. More generally, I provide new evidence that audited financial statements can improve a firm's ability to invest.

Caveats should be noted. First, the context of small banks may reduce the generalizability of my results. Second, despite studying homogenous firms from a single industry segment and using different ways to address potential self-selection bias, I cannot fully rule out confounding omitted variables correlated with audit choice. Finally, like many other studies, this study may be subject to reverse causality or joint determination.

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APPENDIX A

Variable Descriptions

Panel A: Monetary Policy Measures

MP Measures	
The Boschen-Mills Index	The index is available from the website of Prof. Charles L. Weise at: http://www.gettysburg.edu/academics/economics/char_weisehomepage/charles_weise.dot. Prof. Weise updated the index through 2000: Q4. I use the procedures described in Weise (2008) to update the index through 2007: Q2.

Panel B: Variables Used in Tables 2 Through 4

Variable Name	Call Reports Item Name (Item Number)
Total Assets	= Total assets (RCFD2170)
Log of Total Assets (*Ln*(*TA*))	= Natural log of *Total Assets*

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APPENDIX A (continued)

Variable Name	Call Reports Item Name (Item Number)
Liquid Assets	= Total investment securities (RCFD0390) + total assets held in trading accounts (RCFD2146) + federal funds sold and securities purchased under agreements to resell (RCFD1350) From 1993: Q3 and 2001: Q4: RCFD1350 + held-to-maturity securities (RCFD1754) + available-for-sale securities (RCFD1773) + total trading assets (RCFD3545) From 2002: Q1 onward: Federal funds sold and securities purchased under agreements to resell (RCFDC225) + RCFD1754 + RCFD1773 + RCFD3545 In regression tests, *Liquid Assets* is scaled by end-of-period *Total Assets*
Total Loans	= Total loans and leases, net (RCFD2125) From 2001: Q1 onward: Total loans and leases, net (RCFDB529)
C&I Loans	= Commercial and industrial loans (RCFD1766)
Real Estate Loans	= Loans secured by real estate (RCFD1410)
Nonperforming Loans (*NPL*)	= Total loans and lease finance receivables: nonaccrual (RCFD1403) + past due 90 days or more and still accruing (RCFD1407) In regression tests, *NPL* is scaled by end-of-period gross total loans and leases (RCFD1400)
Total Liabilities	= Total liabilities, net of subordinated debt (RCFD2950) + subordinated debt (RCFD3200)
Core Deposits	= Total deposits (RCFD2200) − amount of deposit accounts of more than \$100,000 (RCON2710)[a] From 2006: Q2 onward: RCFD2200 − amount of deposit accounts of more than \$100,000 (RCONF051)
Managed Liabilities	= Amount of deposit accounts of more than \$100,000 (RCON2710) + subordinated debt (RCFD3200) + other borrowed money (RCFD2850) From 1994: Q1 onward: RCON2710 + RCFD3200 + other borrowed money (RCFD3190) From 2006: Q2 onward: Amount of deposit accounts of more than \$100,000 (RCONF051) + RCFD3200 + RCFD3190
Large CDs	= Total time deposits of \$100,000 or more (RCON2604)
Equity	= Total equity capital (RCFD3210)
Quarterly Change in Total Loans (*Loan_Change*)	= The quarterly change in *Total Loans* scaled by beginning of period *Total Loans*
Quarterly Change in Managed Liabilities (*ML_Change*)	= The quarterly change in *Managed Liabilities* scaled by beginning-of-period *Total Loans*
Audit Indicator (*Audited*)	= 1 if the bank is audited (RCFD6724 = 1 or 2)[b]
OBHC	= 1 if the bank is controlled by a one-bank holding company; one-bank holding company affiliation is identified if the bank is owned by a direct (RSSD9379) or regulatory holder (RSSD9348), and that holder owns only one bank
MSA	= 1 if the bank is located in a metropolitan statistical area (RSSD9180 > 0)

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APPENDIX A (continued)

Variable Name	Call Reports Item Name (Item Number)
LowCap	= 1 if the bank's *Equity*-to-*Total Assets* ratio is below 6 percent (Ashcraft 2006)
Loss	= 1 if the bank made losses in the previous quarter; losses are measured based on income before extraordinary items and other adjustments (RIAD4300)
Internal CF	= The sum of income before extraordinary items and other adjustments (RIAD4300) and provision for loan and lease losses (RIAD4230) scaled by beginning-of-period *Total Loans* (Houston et al. 1997)
Other Variables Used in the Regressions:	
GDP_Growth	= The quarterly change in the natural log of national GDP; national GDP is taken from FRED (Series ID: GDP)
CPI_Growth	= The quarterly change in the natural log of the consumer price index (CPI); CPI is taken from FRED (Series ID: CPIAUCNS)
Basel	= A dummy variable for the time period from 1988–1992
State	= A set of state dummies (RSSD9200)
Trend	= A linear time trend, defined as the distance (in years) of observation period from 1988
Quarter	= A set of three quarter dummies

[a] I use the value of total time deposits of $100,000 or more if RCON2710 is missing. Results remain similar if I exclude these cases.
[b] Except in 1988, when the audit indicator was reported in the June Call Report, the March Report is the sole source of information on the most comprehensive level of external auditing work a bank obtained in the previous fiscal year. I extrapolate the value of the indicator to the other quarters of the same fiscal period.
Whenever possible, I follow the data definitions in the Federal Reserve notes on forming consistent time-series.

APPENDIX B

Predicting External Audit Status

This table reports results for the probit regression of banks' audit status (*Audited*). The regression is estimated using annual data. The sample period starts in 1992, when information about a bank's audit status five years ago is available (*PastAudit*). It ends in 2006, corresponding to the last period when the audit status of a bank is measured in the main tests. The other predictors are lagged one period relative to *Audited*. After applying the same sample selection criteria as in the main tests, 78,191 bank-years remain. Two-tailed p-values based on robust standard errors clustered by bank are reported in parentheses.

	***PastAudit* Not Included (1)**			**Adding *PastAudit* (2)**		
Variable	**Coeff. Est.**	**Wald χ^2**	**p-value**	**Coeff. Est.**	**Wald χ^2**	**p-value**
Intercept	1.024	7.236	(0.007)	1.717	17.472	(0.001)
PastAudit				1.808	5936.703	(0.001)
Bank Complexity						
Branches	0.174	23.330	(0.001)	0.101	11.834	(0.001)
Non-Interest Income	4.542	4.973	(0.026)	0.905	0.292	(0.588)
ROA Volatility	14.860	31.472	(0.001)	14.218	20.976	(0.001)
Stakeholders						
Fewer Shareholders	−0.235	38.192	(0.001)	−0.178	31.248	(0.001)
Ln(# of Deposit Accts)	0.268	59.136	(0.001)	0.177	38.938	(0.001)
Ln(# of Employees)	0.404	46.649	(0.001)	0.308	41.216	(0.001)
Bank-Level Variables used in Equation (1)						
ML_Change	0.028	0.706	(0.399)	0.014	0.109	(0.745)
Loan_Change	0.015	0.449	(0.505)	0.073	2.624	(0.105)
Ln(TA)	0.400	58.982	(0.001)	0.270	40.832	(0.001)
OBHC	0.044	1.513	(0.220)	0.002	0.006	(0.937)
MSA	0.147	16.324	(0.001)	0.127	17.306	(0.001)
LowCap	−0.045	0.593	(0.441)	−0.138	4.973	(0.025)
Loss	0.147	11.765	(0.001)	0.132	6.864	(0.009)
NPL	0.211	0.102	(0.746)	−0.099	0.029	(0.868)
Liquid Assets	−0.145	13.280	(0.001)	−0.158	14.056	(0.001)
Internal CF	−0.013	10.890	(0.001)	−0.007	4.752	(0.030)
Loan Portfolio Information						
C&I Loans	0.531	10.120	(0.002)	0.326	5.244	(0.022)
Real Estate Loans	0.418	10.334	(0.001)	0.265	6.503	(0.011)
Individual Loans	0.371	5.334	(0.021)	0.256	3.098	(0.078)
Loan Commitments	0.365	6.334	(0.012)	0.271	4.203	(0.040)
State and year indicators		Yes			Yes	
Number of observations		78,191			78,191	
Proportion audited		0.54			0.54	
	χ^2		p-value	χ^2		p-value
Likelihood ratio test	34,833.12		(0.001)	57,366.15		(0.001)
Score test	29,574.97		(0.001)	46,790.28		(0.001)
Wald test	24,479.25		(0.001)	33,958.07		(0.001)
Percent Concordant (Disconcordant)		82.9 (17.0)			91.9 (8.0)	
Pseudo R^2		0.323			0.531	

In addition, the choice of the explanatory variables follows prior research. Kohlbeck (2005) argues that bank complexity increases the demand for third-party expertise and induces banks to hire external auditors. Thus, I include three variables that are positively associated with bank complexity. First, I add an indicator variable for banks with branches (*Branches*) to assess the impact of geographically dispersed banking operations. Second, I include the ratio of non-interest income to total assets (*Non-Interest Income*) to assess the impact of non-lending bank operations. Third, I add the volatility of return on assets (*ROA Volatility*) because complex operations can result in volatile operational results.

Next, I include three proxies for stakeholders' demand for audits. First, small banks with less dispersed shareholders may be less subject to the demand for an audit, as their shareholders may have more direct oversight of management. To capture this effect, I add an indicator variable for banks that are S corporations, which allow a maximum of only 75 shareholders (*Fewer Shareholders*). A bank's audit choice may also be related to how dispersed its depositor base is. Assuming the number of depositors of a bank is positively related to its number of deposit accounts, I include the number of deposit accounts to capture increased depositors' impact (*Ln(# of Deposit Accts)*). Finally, I consider the influence from employees. Employees invest human capital in their bank and their welfare is tied to the bank's performance, so employees have an interest in the bank's financial position. Assuming employees' influence is related to staff size, I include the number of employees of the bank (*Ln(# of Employees)*) to capture the employees' impact.

The model also includes all the bank-level variables used in Equation (1). As discussed in Sections III and V, these variables are expected to be associated with banks' audit decisions due to the effects of bank growth (*ML_Change* and *Loan_Change*), bank size (*Ln(TA)*), organizational structure (*OBHC*), urban business settings (*MSA*), managers' concerns due to capital inadequacy (*LowCap*), difficult business environments (*Loss*), the bank's ability to generate cash flows (*Internal CF*), and the bank's liquidity management policy (*Liquid Assets*). To assess the impact of different lending operations, I also include four variables related to the composition of the bank's loan portfolio: *C&I Loans*, *Real Estate Loans*, *Individual Loans*, and *Loan Commitments*. Finally, I include a set of state and year indicator variables to control for the effects of different geographical regions and periods.

APPENDIX C

Descriptions of Selected Variables[a]

Variable Name	Call Reports Item Name (Item Number)
PastAudit	= 1 if the bank was audited five years ago
Branches	= 1 if the bank has branches; this information is taken from either the Summary of Deposits from the FDIC (Unit) or Research Information System (Unit)
Non-Interest Income	= The ratio of non-interest income (RIAD4079) to beginning-of-year total assets
ROA Volatility	= The standard deviation of return on assets over the past five years; return on assets is based on the ratio of income before extraordinary items and other adjustments (RIAD4300) to beginning-of-year total assets
Fewer Shareholders	= 1 if the bank is an S corporation (RIADA530)[b]
Ln(# of Deposit Accts)	= Natural log of the number of deposit accounts (RCON2722 + RCON3779); from 2006 onward, natural log of (RCONF050 + RCONF052)
Ln(# of Employees)	= Natural log of the number of full-time employees (RIAD4150)
C&I Loans	= Commercial and industrial loans (RCFD1766) scaled by end-of-year total loans
Real Estate Loans	= Loans secured by real estate (RCFD1410) scaled by end-of-year total loans
Individual Loans	= Loans to individuals (RCFD1975) scaled by end-of-year total loans
Loan Commitments	= Unused commitments (RCFD3423) scaled by end-of-year total loans

[a] Appendix A provides measurement of other variables.
[b] RIADA530 is not available before 1997. To save sample size, I use the value in 1997 for missing values.

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Has the Quality of Accounting Education Declined?

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ABSTRACT: For decades, prominent members of the accounting community have argued that the quality of accounting education is falling. Support for this claim is limited because of a scarcity of data characterizing the constructs of interest. This study is a comparative evaluation of the quality of accounting education from the 1970s to the 2000s using unique data to quantify education quality for accounting and many comparison disciplines. I find that, compared to most other types of college education, accounting education quality has been steady or increasing over the sample period. However, relative to other business degree programs, the evidence is mixed. The quality of students self-selecting non-accounting business degrees has increased while the quality of accounting students has not. The disparity in student quality is not reflected in the pay received by accounting graduates, which has remained stable relative to the pay received by graduates with other business degrees, although this result is likely influenced by regulatory changes during the 2000s, including Sarbanes-Oxley (SOX). Together, the evidence suggests that the quality of accounting education has not declined rapidly over the last four decades, but in the competition among business degree programs for high-quality students, accounting has underperformed.

Keywords: *accounting education; returns to education; accounting major premium; accounting student quality.*

Data Availability: *Data used in this study are available in the Freshman and Senior Surveys of the Cooperative Institutional Research Program's Higher Education Research Institute at the University of California, Los Angeles; the IPUMS-USA database, which is compiled and distributed by the Minnesota Population Center at the University of Minnesota; the National Survey of College Graduates, which is available from the National Science Foundation; and the General Social Survey, which is maintained by the National Opinion Research Center at The University of Chicago.*

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I. INTRODUCTION

For decades, accounting practitioners, academics, and professional bodies have expressed concern that the quality of accounting education is in decline. There is evidence suggesting that current accounting programs fail to teach accounting students many important skills,[1] that educational models that emphasize reasoning and judgment over rote learning may improve accounting educational outcomes (Pincus 1997; Stone and Shelley 1997), and that many accounting educators and practitioners are dissatisfied with the nature of accounting education and the quality of accounting graduates (Albrecht and Sack 2000; Demski 2007; Jennings 2004; Siegel and Sorensen 1994; Weiser 1966; West 2003; Zeff 1989). However, a significant weakness of most existing evidence is that, while it illustrates shortcomings of accounting education, it provides limited information about whether these shortcomings are anomalous when compared against accounting education in the past, against other types of business education, or against university education in general.[2] My study complements prior research because it uses comparative data over an extended period for many disciplines in addition to accounting, which enable me to assess whether weaknesses in modern accounting education are unique to accounting.

This study provides a quantitative, comparative history of accounting education quality. It is distinctive because it uses quantitative indicators of education quality that are available over 40 years for accounting, other business disciplines, and all types of university education. These data enable me to gauge the relative quality of accounting education in the 1970s when it is compared against the quality of contemporaneous alternative types of education, and then examine the magnitudes of changes in the relative quality of accounting education since the 1970s. While there are likely many economically important outcome variables that are sensitive to the quality of accounting education, few have been measured broadly across many fields and consistently over extended periods, such as the 1970s to the 2000s. My dependent variables are measures of the quality of college students pursuing various degrees and measures of the private labor market returns received by recent college graduates across many fields, both of which can be thought of as measures of the attractiveness of accounting education to students and employers. I examine student quality and returns to education not because they comprehensively describe the quality of accounting education; comprehensive measures of the quality of accounting education do not exist. Rather, I examine student quality and returns to education because they are likely sensitive to changes in the relative quality of accounting education and because they are available across many types of education and over long periods of history.

I evaluate the quality of accounting students relative to non-accounting business students and relative to non-business students using surveys of incoming college freshmen available since 1971, as administered by the Cooperative Institutional Research Program of the Higher Education Research Institute (HERI) (University of California, Los Angeles [UCLA] 2013). The HERI freshman surveys ask incoming college students about their abilities, interests, demographics, high school careers, and the fields in which they intend to earn a degree. I consider high-quality college students to be those possessing a number of characteristics that the literature on accounting

[1] Specifically, accounting graduates have been said to have insufficient knowledge of history (Demski 2007; Zeff 1989), knowledge of the economic and social roles of accounting (Gallhofer and Haslam 1996; Weiser 1966; Zeff 1989), ethical training (Armstrong 1987; Jennings 2004; Mathews 2001; Ponemon 1992), leadership skills (Weiser 1966), communication skills (American Accounting Association [AAA] 1986; Andrews and Koester 1979; Gingras 1987; Ingram and Frazier 1980; Mathews 2001; Weiser 1966), or ability to appropriately apply professional judgment, reasoning, and skepticism (AAA 1986; Baxter 1979; Bonk and Smith 1998; Sunder 2010; Zeff 1989).

[2] Throughout this study, I define "business education" or "business degrees" to include degrees in accounting, business administration, finance, international business, marketing, management, and "other business," regardless of whether these degree programs are housed in a university's business school.

education predicts are valuable to students as they attend college and in labor markets. These include academic ability and several measures of their attitudes and "soft skills," including their goals and reasons for attending college, their self-assessed leadership ability, popularity, and ability to understand others. Several of these have been shown to explain incomes among adults, including high school grades (Miller 1998), leadership roles in high school (Kuhn and Weinberger 2005), and popularity in high school (Conti, Galeotti, Mueller, and Pudney 2012). I compare accounting against other business disciplines and against non-business disciplines because I find evidence that accounting degree programs compete for students primarily with other business disciplines, but also face significant competition from non-business disciplines. Further, as detailed in Section II, accounting graduates looking for jobs likewise compete primarily with workers holding non-accounting business degrees, but also face significant competition from workers educated in non-business disciplines.

In general, accounting students were academically strong in high school and are particularly likely to be interested in accounting education because of the perceived financial rewards. Accounting students are particularly unlikely to have chosen the accounting major because it interests them and are less likely to view themselves as leaders, as popular, or as more socially insightful than other students. I compare accounting students against non-business students, who make up more than 76 percent of students and, therefore, can be considered to represent typical college students. I find that the quality of accounting students has remained quite stable when compared against the quality of a typical college student, even improving slightly by several measures when I control for demographics and parental educational attainment. The quality of students in non-accounting business disciplines improved significantly relative to the typical college student.

When I compare changes in the quality of accounting students to changes in the quality of other business students, I find that the quality of accounting students declined from the 1970s to the 2000s by statistically significant magnitudes across all of my measures. Throughout the sample period, accounting students dominated non-accounting business students in terms of academic ability by declining margins, but had weaker soft skills by increasing margins. These results are driven by improvements in the quality of students choosing non-accounting business majors rather than by declines in the quality of students choosing to major in accounting. In terms of magnitudes, my results suggest that out of 100 students, non-accounting business programs increased enrollments of high-quality students by between two and seven students more than did accounting programs. Such estimates of effect sizes are a contribution of this study, but interpreting their economic significance is not straightforward. The extent to which the loss of between 2 percent and 7 percent of high-quality students to other business disciplines (but not non-business disciplines) would trouble employers or department chairs is not known. Evaluating the economic significance of effects of the sizes I document could potentially be a fruitful topic for future research.

My second set of analyses examines the incomes of recently educated workers in U.S. labor markets in order to assess whether the labor market returns to accounting education have changed over time.[3] Incomes in labor markets are likely influenced by a wide variety of forces, including changing economic conditions, as well as workers' intelligence, charisma, years of work experience, geographic location, gender, race, the quantity and quality of their education, and the scarcity of their skills. My tests attempt to isolate the effect of education quality on income in two ways. First, I model income as a function of a number of individual-level controls that are likely

[3] I adopt this approach from the economics literature on the private returns to investment in education as reviewed by Harmon, Oosterbeek, and Walker (2000), Card (2001), and Psacharopoulos and Patrinos (2004).

related to both the choice to pursue an accounting degree and to income. Second, I compare the incomes of subsamples of workers who are likely to be influenced by similar macroeconomic and competitive forces. Specifically, my main tests compare the incomes of young, full-time workers who have comparable amounts of higher education and work in business occupations, after controlling for a number of their characteristics.

By comparing young workers with similar education levels, I limit the impact of varying work experience and education quantity on income. By comparing workers in business occupations, I limit the influence of macroeconomic shifts in supply and demand across varying economic sectors on incomes so that the results are not affected by macroeconomic shocks impacting the incomes of workers in all business occupations similarly. Even with these controls in place, the remaining variation in income is likely influenced by more than simply the quality of the education workers receive. However, I assume that if the quality of accounting education has been "decreasing rapidly" relative to the quality of other types of business education (Albrecht and Sack 2000, 1), then the market will have responded by materially reducing demand and wages, producing changes in relative incomes large enough to be statistically detectible in my tests.

My tests of the relative incomes of accountants use data from IPUMS-USA, a version of the U.S. Decennial Census that is compiled and distributed in machine-readable format by the Integrated Public Use Microdata Series (IPUMS) project of the University of Minnesota (Ruggles et al. 2010). I find no evidence that the relative incomes of accountants were lower in 2010 than in 1970. Instead, the evidence suggests that the annual incomes of accountants did not significantly change relative to the incomes of workers in other business occupations, and increased by between \$4,500 and \$5,150 relative to the annual incomes of workers in non-business occupations. This is not what one would expect to observe if the quality of accounting education declined significantly over this period. Accountants' labor market outcomes in 2010 may have been influenced by the passage of the Sarbanes-Oxley Act (SOX) in 2002. To examine whether the relative returns to accounting education were declining before the passage of SOX, I repeat my tests comparing the relative returns to education for accountants in 1970 and 2000. I find that workers with bachelor's degrees in accounting experienced declines in relative annual pay of \$2,120 (4 percent) between 1970 and 2000 when compared against workers in other business disciplines. Accountants with master's degrees did not experience similar relative declines. This suggests that the relative returns to bachelor's education in accounting may have increased during the 2000s because regulatory changes increased the demand for accounting services. As was the case with student quality, these estimates of effect sizes are new to the literature and assessing their materiality is subjective.

Together, the evidence suggests that the quality of accounting education has remained stable (when using student-quality measures) or has increased (when measured using income) relative to a typical college education. However, the results are mixed when changes in the quality of accounting education are compared against changes in the quality of other types of business education. I find that the quality of non-accounting business students has increased by a fairly large margin over the past four decades, while the quality of accounting students has not changed. This evidence suggests that accounting degree programs have performed poorly in the competition for high-quality students among business degree programs. My evidence on the pay of recent college graduates suggests that the declining quality of accounting students relative to other business students has not resulted in declining relative pay over the sample period for accounting graduates. However, the pay results should be interpreted with caution because they are likely influenced by events in the 2000s, such as the implementation of SOX, that could have increased the demand for accounting labor.

My data and tests have a number of limitations. To assess the quality of accounting students, I use data describing the degree preferences of college freshmen because it is the only data of which I am aware describing student quality by degree that has been available over a significant portion of

my sample period. Because the data are collected at college orientation or registration, they are most informative about the characteristics of students interested in pursuing accounting degrees rather than students graduating with accounting degrees. Using more recent data describing students from HERI's freshman surveys when they become college seniors, I find that (1) freshmen who say they want to major in accounting actually do major in accounting at abnormally high rates, and (2) the only change in student quality between the freshman and senior surveys that is unusual for accounting is for grade-point average (GPA), which improved by a statistically significantly greater amount than it did for an average degree. Its high retention rate suggests that freshman data are more informative about accounting graduates than for other majors. The findings that changes in the quality of accounting freshmen and seniors are either positive (GPA) or statistically insignificant (all other quality measures) suggest that the relative quality of accounting students is stable or improving between their freshman and senior years. Data describing the field of the degrees of workers in U.S. labor markets are not available in any large, publicly available datasets of which I am aware until the 1990s. Therefore, I must infer the field of worker degrees in my main analyses of income. My results are informative about the quality of accounting education only to the extent that I can accurately identify workers who received accounting education and isolate the influence that this education has on income. Robustness tests using more comprehensive data collected recently suggest that my methods for inferring the field of degree in IPUMS-USA identify a high concentration of workers with accounting degrees, but it is possible that my methods were less effective in 1970, when there are no data to test them. Finally, my conclusions depend on whether student quality and returns to education are indicators of the quality of accounting education. As is the case with many survey-based measures, students' academic abilities, motivations, and personality traits, and workers' incomes and other characteristics are self-reported. My comparative approach likely reduces the importance of the potential bias in self-reported measures, but it is possible that school administrators and employers would assess the survey respondents differently than they assess themselves.

I develop my hypotheses in Section II. In Section III, I present my analyses of the relative quality of accounting students, and in Section IV, I present my analyses of the relative incomes of workers with accounting education. Section V discusses conclusions.

II. HYPOTHESIS DEVELOPMENT

This section discusses the literature on accounting education quality with special emphasis on Albrecht and Sack (2000), and explains how my study relates to this literature. I then discuss my outcome variables, student quality and returns to education, and argue that they are distinctively useful indicators of accounting education quality. Next, I provide some evidence regarding the timing of the reported decline in accounting education and regarding appropriate comparison groups against which to evaluate accounting. Finally, I present hypotheses.

Existing literature on the quality of accounting education comes from diverse academic and non-academic sources, draws on a wide range of types of evidence, identifies a wide variety of potential weaknesses in accounting education, and often reaches conflicting conclusions. However, recurring themes from this literature include, first, that that the demands of accounting work have changed due to the evolution of the business environment, and accounting education has not adapted sufficiently to prepare students to meet these new demands (Albrecht and Sack 2000; AAA 1986; Kulesza and Siegel 1996; Arthur Andersen et al. 1989; Schultz 1989; Weiser 1966; West 2003). A second theme is that accounting education has become excessively focused on teaching the application of well-specified rules to unrealistically well-specified business problems while neglecting other important skills (Albrecht and Sack 2000; Baxter 1979; Bloom, Heymann, Fuglister, and Collins 1994, Chapters 6–7; Stone and Shelley 1997; Tinker 1985, xx–xxi; Weiser

1966; West 2003, Chapter 7; Zeff 1989). A final theme is that accounting education lacks intellectual rigor and has become too vocational (Demski 2007; Fellingham 2007).[4]

Some of the evidence supporting these arguments is anecdotal because large sample proxies for the relevant constructs are generally not available. Relevant constructs include the extents to which the business environment has changed, accounting education has adapted to these changes, accounting education has become procedural and rules-centric, and accounting education is intellectually rigorous. In this study, I seek to extend prior literature by identifying indicators of education quality that are quantitative and widely available. The proxies I identify are student quality and relative returns to education. While these proxies are not direct measures of the constructs at the center of the debate about accounting education quality, they are likely highly correlated with these central constructs. Therefore, my measures provide a new perspective for estimating how accounting education quality has changed over time to complement evidence in the prior literature.

My study is most closely related to Albrecht and Sack (2000; hereafter, AS). AS is a study about the future of accounting education commissioned by the Institute of Management Accountants (IMA), the American Institute of Certified Public Accountants (AICPA), the American Accounting Association (AAA), and the Big 5 professional service firms. AS is exceptional because it seeks to analyze the potential consequences of poor-quality accounting education by considering a wide array of related topics and gathering data from large numbers of accounting educators and practitioners using surveys and focus groups. AS find that the business environment has changed significantly in recent years and that many respondents do not believe that accounting education has kept pace with the changes, and the *quantity* of students enrolling in accounting degree programs declined throughout the 1990s. Further, many respondents believe that the *quality* of accounting students has declined as well, and that accounting graduates are paid too little. Finally, many respondents would not major in accounting if they could choose again. The evidence in AS is among the best available for describing the opinions of members of the accounting community about a wide array of education topics. However, while opinion-based evidence is a valuable means of measuring the difficult-to-measure constructs discussed in the literature on accounting education quality, it can potentially be limited by cognitive biases that influence perception (Pearson, Ross, and Dawes 1992; Kahneman 2011). My evidence complements the evidence in AS by drawing on comparative data from other disciplines to aid in the interpretation of the accounting-specific findings and to reduce some of the effects of biased perceptions.[5]

My evidence is narrower than that in AS, but arguably more objective. Specifically, I avoid some of the limitations of the qualitative and anecdotal data used in prior work on accounting education by using data that do not require long-term retrospection from respondents and by performing comparative analyses. Research evaluating the quality of responses to retrospective survey questions has shown that respondents often remember life events surprisingly poorly and

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[4] Deficiencies in accounting education and accounting graduates have also been attributed to poor Ph.D. training producing poor accounting teaching and scholarship (Demski 2007), an emphasis on achieving high pass rates on the uniform CPA exam (AAA 1986; Weiser 1966; Zeff 1989), and a "climate of conformity" created by strict enforcement of financial reporting rules combined with high litigation risk (Zeff 1989, 207).

[5] A number of perceptual biases could potentially have influenced the opinions expressed by participants in AS. For example, AS find that almost 82 percent of surveyed accounting faculty believe that the quality of students majoring in accounting over the last five years has either stayed the same or declined (AS 2000, 21). These assessments are consistent with declines in the quality of accounting students and are also consistent with nostalgia bias or "rosy retrospection" (Mitchell, Thompson, Peterson, and Cronk 1997). As another example, AS find that many accounting academics and practitioners say they would have chosen a college major other than accounting if they could choose again (AS 2000, 33). The regret accountants feel about their educational decisions is consistent with a decline in the attractiveness of accounting careers, but is also consistent with the general phenomenon that a person who learns more about alternative choices that have resulted in good outcomes is more likely to regret their own prior choices (Kahneman 2011, 346–349; Schwartz 2004, Chapter 7).

that the accuracy of their memories degrades as the time since the event increases (Loftus, Smith, Klinger, and Fiedler 1992). While AS respondents evaluate changes in accounting education over five- or ten-year windows, I use data that require more limited retrospection and are primarily factual rather than opinion-based. The most demanding recollections used in my tests require college freshmen to recall their high school GPA and workers to recall their annual income for the prior year. In addition, my tests control for those cognitive biases that are general to all people because they evaluate the information provided by accountants by comparison against comparable information collected from similar, non-accountant respondents. By comparing the data across fields, I seek to remove the influence of biases that impact the responses of accountants and non-accountants similarly.[6]

My tests are relevant only to the extent that my student quality and returns to education outcome variables are meaningful indicators of education quality. Much of the existing literature on accounting education quality focuses on describing deficiencies in accounting education and making proposals to resolve them (Accounting Education Change Commission [AECC] 1996; Kulesza and Siegel 1996; Siegel and Sorensen 1994; Siegel, Sorensen, Klammer, and Richtermeyer 2010), but there is limited information about the consequences of failure to resolve the deficiencies. As a result, student quality and returns to education are not central themes in this literature. However, in discussions of the consequences of declining education quality, both student quality and returns to education are listed among important economic outcomes that are likely sensitive to changes in the quality of accounting education.

Prior work has explicitly linked student quality and returns to education to the quality of accounting education. AS list "the number and quality of students electing to major in accounting is decreasing rapidly" as their first indicator that "accounting education today is plagued with many serious problems" (AS 2000, 1). Demski (2007, 154) perceives excessive myopia and vocationalism in accounting education, which, he argues, "provides an unmistakable (and embarrassing) signal to the university in terms of our scholarly commitment and in terms of student self-selection into our curricula." Sunder (2009, 108) argues that accounting education focuses too much on teaching the application of standards and, as a consequence, "attracts less talent to accounting programs and, ultimately, to the accounting profession" (see, also, Sunder 2010, 109; Sunder 2011, 10). Regarding the sensitivity of pay to education quality, AS argue "it seems very unlikely that starting salaries for our graduates will be increased without action on our part to make those graduates more valuable" (AS 2000, 2) and that "there may very well be a stronger relationship between the 'value add' of an accounting education and the starting salaries of our graduates than we are willing to admit" (AS 2000, 60). Non-accounting sources also support a link between pay and education quality. Becker's (1964) seminal work on "human capital" shows that there are significant economic returns to increasing quantities of education. Recent work in this literature suggests that, in addition to education *quantity,* income is sensitive to education *quality.* There are large income disparities between college graduates with different majors, even when the quantity of education is the same

[6] The power of comparison to deal with common cognitive biases can be illustrated by considering again the finding in AS that many accounting academics and professionals say that they would not pursue an accounting degree if they were choosing again. Evaluating this piece of evidence is difficult because it is not clear whether this is unique to the accounting profession. In an untabulated analysis of a survey dataset called the General Social Survey (GSS) (Smith, Marsden, Hout, and Kim 2010), I find that the percentages of accountants that reported being "very satisfied" with their jobs was 37.5 percent in the 1970s and 34 percent in the 2000s. The job satisfaction results for accountants become more meaningful when they are compared against a benchmark. When I examine all non-accountants in the GSS, I find that the percentages that reported being "very satisfied" with their jobs were 38.5 percent in the 1970s and 28.3 percent in the 2000s. When compared against people in other disciplines, the satisfaction levels reported by accountants in the GSS do not appear to be unusually low. Without information on relevant comparison groups made up of non-accountants, accounting-specific evidence on job satisfaction is subject to misinterpretation.

and after statistically correcting for differences in student ability (Arcidiacono 2004). In addition, Brewer, Eide, and Ehrenberg (1999) show that workers with degrees from the highest-quality colleges (using selectivity and public versus private control as proxies for quality) earn more than workers with degrees from other colleges after controlling for ability. These findings are consistent with AS in that they suggest that income is sensitive to education quality, whether defined in terms of the quality of the university attended or the student's major.

Designing empirical tests examining whether the quality of accounting education has declined requires that I identify the timing of the decline. Mautz (1965, 309) is the first prominent observer of whom I am aware to argue that some "unfortunate results" of excessively procedural accounting instruction include that "accounting students tend to develop little ability to reason" and do not learn to "think critically about accounting." Sundem (1999) argues that accounting education quality began to decline in the mid-1980s. I conducted a Proquest search for articles in the electronically searchable archives of *The Accounting Review* (1917–2013) and the *Journal of Accountancy* (1905–2013), which were in print during most of the 20th century, for articles in which the words "accounting" and "education" appear near each other and the words "quality" and "education" also appear near each other (within five words). The results suggest that discussion of the quality of accounting education first occurred in these journals in the 1940s, but was most intense during the 1960s–1990s, during which 86 percent of the 105 articles meeting the search criteria were published. Concern about accounting education quality has continued into the 2000s (Demski 2007; Fellingham 2007; Jennings 2004; Mathews 2001; Siegel et al. 2010; Sunder 2009, 2010, 2011; West 2003). I conclude that concern about the quality of accounting education was most intense between the mid-1960s and the late 1990s, perhaps topping out in the late 1980s, and continues today.

As another means of evaluating the timing of the decline in the quality of accounting education, I examine the rate of proliferation in accounting standards since the 1930s. If the proliferation of accounting standards is an important cause of poorer accounting education (Baxter 1979; Sunder 2010; West 2003; Zeff 1989), then I expect to see that its increase coincided with the increasing discussion of the quality of accounting education in the literature. Figure 1 shows data from Google's n-gram database.[7] An n-gram is a phrase that is n words long. The database is a collection of n-grams, along with their frequencies per year, as they appear in Google's collection of millions of digitized American English books. Google n-gram data have been used in prior research to characterize cultural trends (Michel et al. 2011). Figure 1 shows the frequencies at which the two-grams "Accounting Standards" and "auditing standards" appear per million n-grams of the same length at five-year intervals since 1930, as well as a line labeled "standard-setting bodies" showing the sum of the appearances of the names of major financial reporting standard-setters; specifically, the n-grams "Committee on Accounting Procedure," "Accounting Principles Board," and "Financial Accounting Standards Board" in the n-gram database.[8] All three lines in Figure 1 are upward-sloping, with the greatest increases occurring between 1965 and 1980. If accounting standardization affects the quality of accounting education, then it may do so with a lag of unknown

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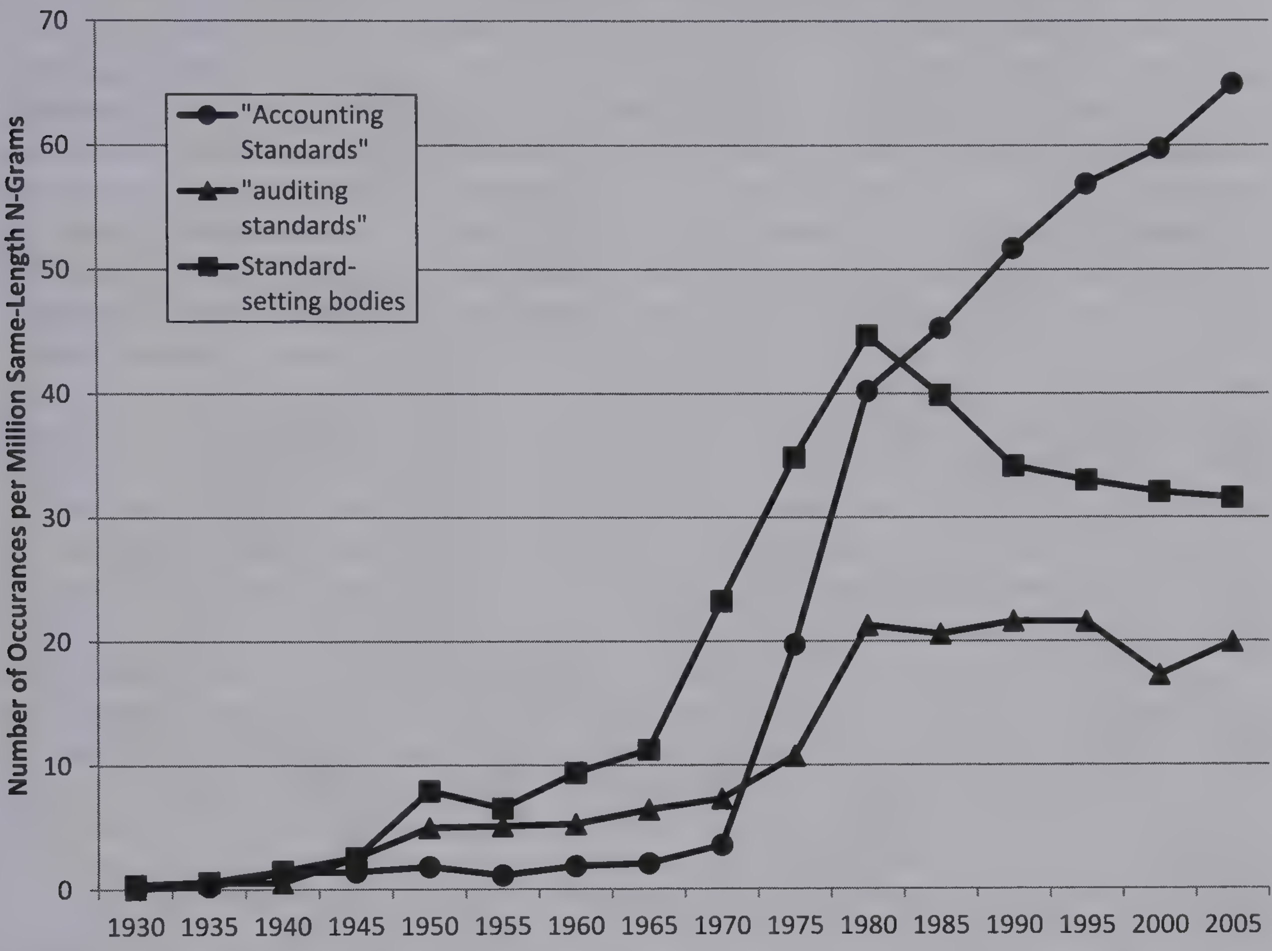
[7] The results of this search in Google's n-gram viewer can be viewed at: https://books.google.com/ngrams/graph?content=Accounting+Standards%2Cauditing+standards%2CCommittee+on+Accounting+Procedure+%2B+Accounting+Principles+Board+%2B+Financial+Accounting+Standards+Board&year_start=1930&year_end=2008&corpus=17&smoothing=3&share=&direct_url=t1%3B%2CAccounting%20Standards%3B%2Cc0%3B.t1%3B%2Cauditing%20standards%3B%2Cc0%3B.t1%3B%2C%28Committee%20on%20Accounting%20Procedure%20%2B%20Accounting%20Principles%20Board%20%2B%20Financial%20Accounting%20Standards%20Board%29%3B%2Cc0

[8] n-gram counts are noisy. To reduce the noise in the reported n-gram frequencies, values in Figure 1 are smoothed by averaging the value for the year of the observation with the three years before and the three years after. In addition, the n-gram database distinguishes between capitalized and uncapitalized letters. When constructing Figure 1, I searched each term when capitalizing the first letter of both words, neither word, or only the first word. Figure 1 shows n-gram frequencies for "Accounting Standards" with both words capitalized and "auditing standards" with neither capitalized because these were the variants that resulted in the highest n-gram frequency counts.

FIGURE 1
Appearances in American English Books of the N-Grams "Accounting Standards," "auditing standards," and the Names of Accounting Standard-Setting Bodies (Aggregated)



Data in this figure come from Google's database of American English n-gram frequencies. Each line shows values taken at five-year intervals and smoothed by averaging data for the indicated year with values for the three years before and the three years after. The line labeled "Accounting Standards" represents the number of times this two-gram appeared during a given year. The line labeled "auditing standards" represents the number of times this two-gram appeared during a given year. The line labeled "Standard-setting bodies" represents sums of the appearances of the names of accounting standard-setters in a given year; specifically, "Committee on Accounting Procedure," "Accounting Principles Board," and "Financial Accounting Standards Board."

length. I conclude from these analyses of the accounting literature and accounting standardization that tests examining the quality of accounting education should ideally encompass the years between the mid-1960s and the 2000s because these are the years during which the evidence and theory suggest that the quality of accounting education is most likely to have declined.

When accounting education quality is assessed against an explicit comparison group in prior literature, this group is most often made up of other types of business education. When the goal is to evaluate the relative quality of accounting education, comparison groups should be those competing with accounting degree programs for high-quality students, and with accounting graduates for jobs. While it seems likely that the accounting profession competes most directly with other business fields, it likely faces some amount of competition from non-business disciplines as well. I am aware of no existing evidence on the extent to which accounting competes with business versus non-business disciplines. Therefore, I conduct exploratory data analyses in the HERI Senior dataset and a dataset called the National Survey of College Graduates (NSCG), which is composed of millions

of observations of college graduates in the U.S., including data on the field of their highest degrees and their occupations (National Science Foundation [NSF] 2010). I use these sources to guide selection of my comparison groups.

I begin by examining students in the HERI Senior database for the years 1994–1999 to better understand the nature of the competition for college students between degree programs.[9] Specifically, I examine freshmen who indicated an interest in earning an accounting degree, but as seniors had chosen to major in something other than accounting. I also examine any students who indicated an interest in non-accounting degrees as freshmen, but as seniors had chosen to major in accounting. By examining these major-switching students, I can see whether the competitive environment for accounting students is isolated to the business school or encompasses the whole university. I find 3,441 seniors who, as freshmen, were interested in pursuing an accounting degree; 1,448 of these students eventually chose a major other than accounting. Of these degree-switchers, 851 (59 percent) switched to another business degree, while 597 (41 percent) switched to a non-business degree. I find that 1,807 seniors who, as freshmen, were interested in pursuing a non-accounting degree had switched to the accounting major by the time they were seniors. Of these, 958 (53 percent) switched from business disciplines and 849 (47 percent) switched from non-business disciplines. I conclude that while a majority of students switching into and out of accounting degree programs come from or leave to go to other business degree programs, a substantial number also come from or leave to non-business degree programs.

I then examine workers under 31 years old in the NSCG surveys conducted in 1993, 2003, and 2010 to see into which occupations workers with accounting degrees enter. Within these occupations, I also analyze what types of non-accounting degrees are held by the labor market competitors of accounting graduates. I find that between 71 percent and 73 percent of students graduating with accounting degrees enter the NSCG's "accountants, auditors, and other financial specialists" occupation. Within this occupation, accounting degree holders make up between 40 percent and 49 percent of the college-educated workforce under 31 years old. Of the remaining college-educated members of this occupation, between 59 percent and 69 percent hold business degrees, while between 31 percent and 41 percent hold non-business degrees. As with the competition among degree programs for accounting students, the market in which accounting graduates compete for jobs is characterized primarily by competition against recipients of other business degrees, but there is also substantial competition with non-business degree recipients. This is true within the accounting occupation, into which about 70 percent of accounting graduates enter after graduation. The remaining 30 percent of accounting graduates enter into a wide variety of occupations where they compete primarily with workers holding non-business degrees.[10]

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[9] Many surveys do not randomly sample their population(s) of interest. Instead, they employ a wide array of survey sampling methodologies, often in an effort to oversample important, but small, groups that may not receive enough coverage if the sampling were strictly random. Such methods help the survey-takers to thoroughly cover important subpopulations, but they can bias population-level inferences if oversampling is not accounted for in the analysis. To enable researchers to debias survey analyses, most survey datasets include survey-weighting variables whose purpose it is to describe the probability of sampling each observation given the non-random survey sampling methodology. These weighting variables can then be used to adjust or weight observations so that unbiased population-level inferences can be made. In all of the analyses to follow, I discuss when and how I use survey sampling weights. The HERI Senior database does not include a variable describing survey sampling probability weights. As a consequence, the analyses I report using this database use unweighted methods.

[10] These analyses involve comparing the proportions of workers in the "accountants, auditors, and other financial specialists" occupation holding accounting, business, or non-business degrees. When calculating these proportions, I weight observation to account for survey sampling probabilities using the NSCG "wtsurvy" variable.

When I examine the quality of accounting freshmen, I assume that students choose college majors under conditions of uncertainty about their own "tastes for schooling," abilities, and likely future earnings given their choice of major (Altonji 1993; Altonji, Blom, and Meghir 2012). There is evidence that college freshmen choose majors based in part on their uncertain assessments of their interest and aptitude in a subject (Malgwi, Howe, and Burnaby 2005), and I assume that these assessments are influenced by the nature of coursework in the field. The nature of college-level accounting courses could influence the self-assessed interest of new college freshmen in a given field if, for example, fields of study develop reputations that are influenced by the nature of training in the field.[11] These assumptions are important in this study because they imply that the reputation of accounting education could be damaged if accounting coursework has become increasingly unappealing to high-quality students. Reputational damage could discourage high-quality incoming college freshmen from pursuing accounting education. This leads to my first hypothesis, which I state in alternative form:

H1: The quality of college freshmen expressing interest in earning an accounting degree declined between the mid-1960s and 2000s relative to students in other business majors and non-business majors.

Following the economics literature on human capital, I assume that investment in education quality (Arcidiacono 2004; Brewer et al. 1999) raises worker productivity and wages. This assumption implies that, all else equal, if the quality of accounting education declines over a period of time relative to the quality of other available types of education, then demand for accounting labor will decline relative to demand in other fields. Assuming constant or increasing supply, wages of workers with accounting education will then fall relative to workers with similar levels of education in related fields. This leads to my second hypothesis, again in alternative form:

H2: The incomes of workers with accounting education declined between the mid-1960s and 2000s relative to the incomes of workers with non-accounting business education and non-business education.

As an initial perspective on my assumption that the supply of accounting labor has been constant or increasing over my sample period, Figure 2 shows the percentile rank of the size of the accounting profession relative to all other U.S. occupations from 1850 to 2010. I estimate it using census data from IPUMS-USA. It shows that the relative size of the accounting profession rapidly increased from about the 30th percentile to the 90th percentile from 1870 to 1930 and then remained at about the 90th percentile through 2010. These data suggest that the relative size of the accounting profession has either increased or remained stable over nearly all of the last century and a half. Sections III and IV describe tests of the hypotheses developed in this section.

III. QUALITY OF ACCOUNTING STUDENTS

In this section, I describe tests of H1, that the quality of college freshmen expressing interest in earning an accounting degree declined between the mid-1960s and the 2000s relative to students in other business majors and non-business majors.

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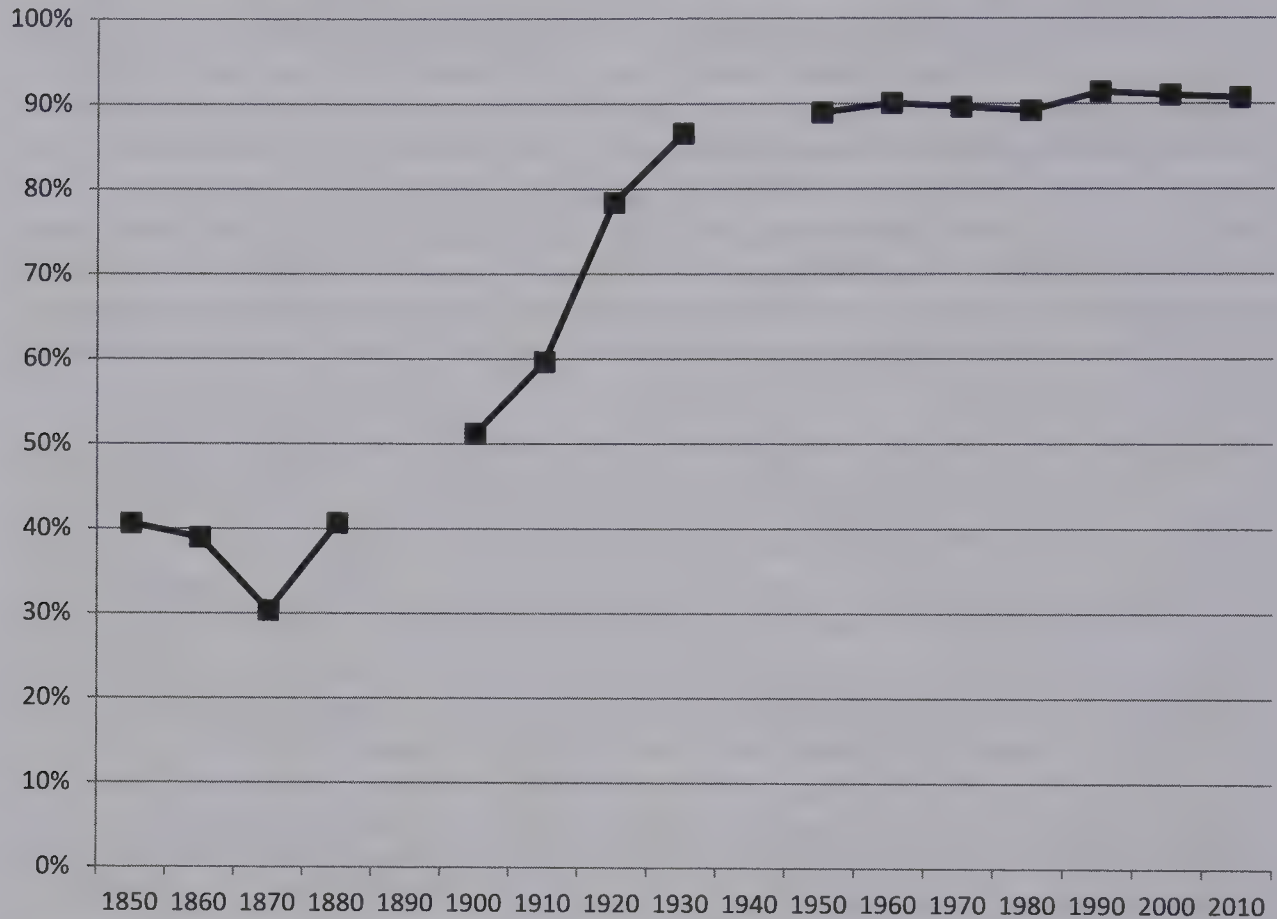
[11] That fields of study can develop reputations is supported by the literature on occupational prestige. This literature shows high concordance between the occupational prestige and socioeconomic "desirability" rankings of occupations provided by independent survey respondents who were drawn from nationally representative samples of the U.S. population. These results suggest that reputational knowledge about a large collection of fields of study is distributed broadly across the population (Featherman and Hauser 1976; Hodge, Siegel, and Rossi 1964).

FIGURE 2
Size Percentile of the "Accountants and Auditors" Occupation among Census Occupations



This figure is constructed using data from IPUMS-USA. Data from 1890 are missing from IPUMS-USA because the original Census Bureau files were destroyed in a fire. Data from 1940 are missing because in that year, the Census Bureau aggregated the "bookkeepers" and "accountants and auditors" occupations.

Data and Models

I use data describing college freshmen that are collected by the Higher Education Research Institute's (HERI) Cooperative Institutional Research Program (CIRP), as administered by the School of Education and Information Studies at UCLA.[12] Data from HERI's college freshman surveys for 1971 to 1999 are available at no charge to researchers who register with CIRP. For my tests, I use the complete database to characterize students during the period 1971–1980. To characterize students during the 2000s, I use a subset of recent HERI data that I purchased after receiving approval from HERI to use them in this study.[13] The HERI freshman survey is administered at hundreds of two-year and four-year colleges each year to incoming freshmen before they begin classes.[14] It describes student values, attitudes, beliefs about cultural topics and themselves, prior achievements, and education and career plans, as well as their demographics, any financial aid they received, and family characteristics.

[12] For more information about HERI, see: http://www.heri.ucla.edu/abtcirp.php
[13] Specifically, HERI provided me with all observations describing students expressing interest in business majors and a large random sample of students interested in non-business majors for each of the years 2000, 2002, 2004, 2006, 2008, and 2010.
[14] A table of participating universities is available at: http://www.heri.ucla.edu/researchers/parthist/TFS.Participation.History.pdf

I search HERI variables to find indicators of student quality that are listed as important in the accounting literature on education quality and that are available over significant portions of the sample period. I include seven variables in my tests, one representing academic ability and the rest representing "soft skills" that accounting students may lack. I use high school GPA as an indicator of academic ability. I recode GPA from an idiosyncratic scale in HERI to be on a traditional four-point scale. I use student responses to a question about whether they are attending college "to learn more about things that interest me" as a measure of passion for a chosen subject. I use student responses about the importance of the goals of "becoming an authority in my field," which I consider to be a measure of ambition and professional achievement, and "being very well off financially," which could be interpreted either as a positive indicator of student quality, indicating practicality and ambition, or a negative indicator suggesting that a student may lack passion for a chosen major and choose to pursue it based on a field's reputation as financially rewarding. Because of this ambiguity, my tests on the "being very well off financially" variable are non-directional. Finally, I use student assessments about how they compare with their peers in terms of leadership ability, popularity, and "understanding of others," which I consider to be a measure of emotional maturity or empathy.[15] Because the measures of student quality other than GPA are reported using rough categorical scales with differences between response categories representing ordinal differences, but not likely precise differences in magnitude, I recode them into indicator variables that are equal to 1 for high responses, and 0 otherwise. I consider high responses to be those in the categories that approximate the top 50 percent of responses in the sample.

Results

Table 1 shows mean values for each student-quality measure during the periods 1971–1980 and 2000–2010 for accounting students, non-accounting business students, and non-business students, as well as unweighted and survey probability-weighted observation counts for each category of students. Mean values reported in Table 1 are weighted averages, with each observation weighted by its survey sampling probability weight. Table 1 also shows, for accounting students and non-accounting business students, the percentile location of their value among all college majors in parentheses. Average high school GPAs increased over time for all students, consistent with inflation in high school grades. Table 1 shows that accounting students have had a fairly stable set of characteristics when compared against all other majors. They had average high school GPAs (42nd and 47th percentiles), viewed pursuing their interests as particularly unimportant (second and sixth percentiles), were somewhat more interested in "becoming an authority" than average students (65th and 62nd percentiles), gave themselves relatively low scores for leadership ability (23rd and 21st percentiles) and "understanding of others" (17th and 15th percentiles), average scores for popularity (47th and 51st percentiles), and listed becoming financially well off as a particularly important goal (90th and 92nd percentiles). The largest relative change across these measures is the 5 percentile increase in high school GPA.

Table 1 also allows me to characterize non-accounting business students. They have low average high school GPAs, viewed pursuing their interests as unimportant, viewed becoming an authority as an important goal at similar rates as typical students in the 1970s, but at much higher rates than average in the 2000s. They also viewed themselves as above-average leaders and as popular, but with somewhat below-average ability to understand others, and were very interested in

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[15] Responses to the questions about attending college to learn more about interests, becoming an authority, and being well off financially are coded on the scale 1 = not important; 2 = somewhat important; and 3 = very important. Responses to the questions about leadership, popularity, and empathy are coded on the scale 1 = lowest 10 percent; 2 = below average; 3 = average; 4 = above average; and 5 = highest 10 percent.

TABLE 1

Descriptive Statistics and Observation Counts for Student-Quality Measures in the HERI Freshman Database

	Means					Observation Counts					
						Accounting		Business		Non-Business	
Years	Accounting		Business		Non-Business	n	n-Weighted	n	n-Weighted	n	n-Weighted
H.S. GPA											
1971–1980	3.144	(42%)	2.964	(11%)	3.171	70,504	472,550	146,171	958,957	1,288,341	7,941,972
2000–2010	3.385	(47%)	3.305	(24%)	3.420	44,851	171,396	276,115	992,101	463,569	1,644,355
Interest											
1971–1980	0.638	(2%)	0.660	(5%)	0.774	47,103	313,307	102,068	664,421	767,208	4,700,715
2000–2010	0.662	(6%)	0.721	(17%)	0.790	37,762	136,786	232,418	794,502	393,806	1,335,324
Authority											
1971–1980	0.324	(65%)	0.305	(50%)	0.285	69,668	467,179	144,349	946,445	1,277,502	7,870,597
2000–2010	0.224	(62%)	0.258	(84%)	0.188	43,983	168,078	271,268	973,938	456,558	1,617,842
Leadership											
1971–1980	0.419	(23%)	0.510	(65%)	0.475	27,219	181,593	55,341	358,110	518,837	3,155,554
2000–2010	0.542	(21%)	0.679	(82%)	0.604	36,156	145,416	222,854	830,045	374,790	1,376,974
Popularity											
1971–1980	0.333	(47%)	0.409	(87%)	0.342	27,081	180,781	55,041	356,321	516,238	3,140,065
2000–2010	0.381	(51%)	0.514	(94%)	0.371	27,847	113,664	172,987	648,953	295,226	1,113,219
Understanding											
1971–1980	0.627	(17%)	0.660	(38%)	0.700	27,181	181,392	55,272	357,861	519,126	3,157,397
2000–2010	0.581	(15%)	0.656	(49%)	0.668	36,134	145,292	222,741	829,422	374,591	1,376,681
Rich											
1971–1980	0.234	(90%)	0.266	(98%)	0.139	63,866	429,219	132,850	873,143	1,147,046	7,070,802
2000–2010	0.520	(92%)	0.517	(96%)	0.362	44,004	168,195	271,442	974,249	456,611	1,618,199

Data summarized in this table come from the HERI Freshman database, a collection of survey data describing incoming college freshmen. Columns labeled "Accounting" show means and percentiles (in parentheses) for students interested in majoring in accounting. Columns labeled "Business" show means and percentiles (in parentheses) for students interested in majoring in non-accounting business disciplines. Columns labeled "Non-Business" show means for all students not interested in pursuing a business degree. Columns labeled "n" show observation counts, and columns labeled "n-Weighted" show observation counts weighted by each student's sampling probability weight.

(continued on next page)

TABLE 1 (continued)

Variable Definitions:
H.S. GPA = high school grade-point average;
Interest = proportion of students rating "learning more about things that interest me" as a "very important" motivation for attending college;
Authority = proportion of students rating "becoming an authority in my field" as a "very important" or "essential" goal;
Leadership, *Popularity*, and *Understanding* = proportions of students rating themselves as above average relative to other people their age along each of these personal characteristics; and
Rich = proportion of students rating "being very well off financially" as a "very important" or "essential" goal.

TABLE 2

Comparison of Changes from the 1970s to 2000s in the Quality of Students Interested in Accounting, Non-Accounting Business, and Non-Business Majors

	Changes				Difference between Changes	
	Accounting	Business	Non-Business	Pred. Sign	Accounting vs. Business	Accounting vs. Non-Business
H.S. GPA	0.241	0.341	0.250	–	−0.099***	−0.009**
Interest	0.024	0.060	0.016	–	−0.036***	0.009††
Authority	−0.101	−0.047	−0.097	–	−0.054***	−0.004
Leadership	0.123	0.169	0.129	–	−0.046***	−0.006
Popularity	0.049	0.106	0.029	–	−0.057***	0.020†††
Understanding	−0.046	−0.004	−0.033	–	−0.041***	−0.013**
Rich	0.286	0.251	0.223	?	0.036***	0.063***

***, ** Signify statistical significance at the 0.01 and 0.05 levels, respectively.

†††, †† Signify statistical significance at the 0.01 and 0.05 levels, respectively, but with a sign that is opposite predictions.

For directional predictions, significance levels are one-tailed, and for non-directional predictions, they are two-tailed. Data summarized in this table come from the HERI Freshman database, a collection of survey data describing incoming college freshmen. Columns labeled "Accounting," "Business," and "Non-Business" show changes in the mean values for each student-quality measure over time for the indicated subpopulation. The column labeled "Accounting vs. Business" shows the value in the "Accounting" column less the value in the "Business" column, while the column labeled "Accounting vs. Non-Business" shows the value in the "Accounting" column less the value in the "Non-Business" column. Statistical significance is calculated using survey-weighted Wald tests.

Variable Definitions:

H.S. GPA = high school grade-point average;

Interest = proportion of students rating "learning more about things that interest me" as a "very important" motivation for attending college;

Authority = proportion of students rating "becoming an authority in my field" as a "very important" or "essential" goal;

Leadership, Popularity, and *Understanding* = proportions of students rating themselves as above average relative to other people their age along each of these personal characteristics; and

Rich = proportion of students rating "being very well off financially" as a "very important" or "essential" goal.

becoming financially well off. In contrast to accounting students, whose characteristics remained quite stable from the 1970s to the 2000s relative to typical students, non-accounting business students significantly improved. I calculate the percentiles for non-accounting business majors by treating them like a single major and ranking them against all other college majors, and find that they are characterized by improved high school GPAs (11th to 24th percentiles), increasing interest in their major (5th to 17th percentiles), increasing interest in becoming an authority (50th to 84th percentiles), increasing self-assessed leadership skills (65th to 82nd percentiles), popularity (87th to 94th percentiles), and understanding of others (38th to 49th percentiles). The only stable measure is their interest in becoming financially well off, which was consistently high (98th and 96th percentiles). However, even after these improvements, the academic ability of non-accounting business students was still significantly lower than the academic ability of accounting students (24th versus 47th percentiles).

Table 2 shows changes in the Table 1 values from the 1970s to the 2000s for accountants, non-accounting business majors, and non-business majors in the columns labeled "changes." As the main tests of H1, I report comparisons of the magnitudes of these changes in accounting students versus non-accounting business students and accounting students versus non-business students in

the columns in Table 2 labeled "Difference between Changes." The survey-weighted Wald test results indicate that the relative quality of accounting students declined by all six quality measures for which I make a directional prediction, and that they became relatively more interested in becoming financially well off.[16] The largest decline relative to non-accounting business students was a 0.057 decline in students' self-assessed popularity.[17] This result can be interpreted to mean that the change in the proportion of accounting students rating themselves as more popular than average was less favorable by 5.7 percent (or about six out of 100 students) than the change for non-accounting business students. The results are highly statistically significant (the large sample ensures high statistical power) and suggest that accounting degree programs in the 2000s attracted between three and six fewer high-quality students out of 100, relative to non-accounting business programs, than they did in the 1970s. When accounting students are compared to non-business students in Table 2, the results are mixed and mostly of small magnitudes. Accounting student quality declined by statistically significant amounts in terms of high school GPAs and self-assessed understanding of others, and increased in terms of student interest in the field and self-assessed popularity. The largest of these disparities is 2 percent. Accounting students' interest in being financially well off increased by 6.3 percent more than non-business students' interest.[18]

The univariate comparisons in Tables 1 and 2 do not account for differences in the demographic and familial characteristics of students across degree programs. Many institutions have actively sought to increase enrollments of minorities and first-generation college students. Attracting these students to a given institution or degree program is likely viewed as desirable even though these students may have disadvantages in terms of some quality measures, such as high school GPA. To control for demographic and familial differences between students, I rerun the analyses in Tables 1 and 2 after narrowing the sample to include only white male students with college-educated parents and present the results in Tables 3 and 4. Many of the detailed results in Tables 3 and 4 differ from those in Tables 1 and 2, but the conclusions are similar. Accounting student quality was stable or improving by small magnitudes relative to non-business students, as high school GPA, leadership, and popularity improved significantly.[19] Non-accounting business student quality improved relative to the quality of non-business students. The quality of accounting students declined uniformly when it is compared to non-accounting business students.[20] Even after their relative improvements, non-accounting business students had lower high school GPAs than accounting students (36th and 55th percentiles).

The results with respect to H1 are mixed. The quality of accounting students declined between the 1970s and the 2000s relative to other business students, consistent with H1. However, this result is driven by significant improvements in non-accounting business student quality rather than by declines in accounting student quality. Changes in the quality of accounting students were mixed

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[16] In a robustness test, I repeat these analyses without recoding the HERI ordinal variables into dummy variables. The results remain the same.

[17] The decline in relative high school GPAs of −0.099 appears to be the largest magnitude decline because GPA is reported on a 0- to 4-point scale, while the other measures are all percentages bound between 0 and 1. When it is transformed to be on the same scale as the other measures, the relative decline in GPA was −0.025 percent.

[18] In a robustness test, I repeat these analyses without recoding the HERI ordinal variables into dummy variables. The results show that the quality of accounting students improved relative to non-business students in terms of self-assessed popularity, but declined in terms of leadership ability. Contrary to the main tests, accounting students became relatively less interested in becoming financially well off.

[19] In a robustness test, I repeat these analyses without recoding the HERI ordinal variables into dummy variables. I find that the results are somewhat weaker. Relative to non-business students, accounting students improved in terms of self-assessed popularity and became relatively less interested in becoming rich.

[20] In a robustness test, I repeat these analyses without recoding the HERI ordinal variables into dummy variables. The results are nearly identical. The only change is that changes in students' desire to be financially well off were not significantly different.

TABLE 3

Descriptive Statistics and Observation Counts for Student-Quality Measures among White Male Students with College-Educated Parents in the HERI Database

	Means					Observation Counts					
						Accounting		Business		Non-Business	
Years	Accounting		Business		Non-Business	n	n-Weighted	n	n-Weighted	n	n-Weighted
H.S. GPA											
1971–1980	3.085	(44%)	2.913	(22%)	3.188	10,816	66,794	35,217	208,554	221,331	1,161,275
2000–2010	3.396	(55%)	3.305	(36%)	3.447	8,868	33,684	75,225	265,390	87,274	305,742
Interest											
1971–1980	0.596	(5%)	0.635	(13%)	0.749	7,028	42,334	23,893	140,130	132,078	688,159
2000–2010	0.608	(9%)	0.681	(28%)	0.769	7,398	26,681	63,514	214,906	74,725	250,839
Authority											
1971–1980	0.345	(61%)	0.321	(56%)	0.315	10,695	65,956	34,871	206,616	219,819	1,152,360
2000–2010	0.200	(56%)	0.231	(70%)	0.180	8,712	33,130	74,024	260,837	86,117	301,808
Leadership											
1971–1980	0.505	(24%)	0.577	(59%)	0.573	4,166	25,348	13,428	76,555	90,080	460,459
2000–2010	0.617	(26%)	0.732	(71%)	0.664	7,174	28,610	61,096	223,717	70,523	256,478
Popularity											
1971–1980	0.424	(58%)	0.479	(78%)	0.409	4,162	25,314	13,352	76,143	89,737	458,911
2000–2010	0.492	(60%)	0.615	(86%)	0.442	5,388	22,077	47,549	175,788	55,493	208,107
Understanding											
1971–1980	0.629	(18%)	0.661	(39%)	0.686	4,160	25,311	13,397	76,396	90,065	460,351
2000–2010	0.582	(24%)	0.653	(54%)	0.640	7,171	28,588	61,052	223,603	70,452	256,254
Rich											
1971–1980	0.271	(88%)	0.289	(92%)	0.156	9,719	59,871	31,917	189,407	198,079	1,038,029
2000–2010	0.495	(89%)	0.495	(89%)	0.312	8,725	33,175	74,084	260,894	86,103	301,695

Data summarized in this table come from the HERI Freshman database, a collection of survey data describing incoming college freshmen. Columns labeled "Accounting" show means and percentiles (in parentheses) for students interested in majoring in accounting. Columns labeled "Business" show means and percentiles (in parentheses) for students interested in majoring in non-accounting business disciplines. Columns labeled "Non-Business" show means for all students not interested in pursuing a business degree. Columns labeled "n" show observation counts, and columns labeled "n-Weighted" show observation counts weighted by each student's sampling probability weight.

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TABLE 3 (continued)

Variable Definitions:
H.S. GPA = high school grade-point average;
Interest = proportion of students rating "learning more about things that interest me" as a "very important" motivation for attending college;
Authority = proportion of students rating "becoming an authority in my field" as a "very important" or "essential" goal;
Leadership, *Popularity*, and *Understanding* = proportions of students rating themselves as above average relative to other people their age along each of these personal characteristics; and
Rich = proportion of students rating "being very well off financially" as a "very important" or "essential" goal.

TABLE 4

Comparison of Changes from the 1970s to the 2000s in the Quality of White Male Students with College-Educated Parents Interested in Accounting, Non-Accounting Business, and Non-Business Majors

	Changes				Difference between Changes	
	Accounting	Business	Non-Business	Pred. Sign	Accounting vs. Business	Accounting vs. Non-Business
H.S. GPA	0.311	0.392	0.260	–	–0.080***	0.052†††
Interest	0.012	0.046	0.019	–	–0.033***	–0.007
Authority	–0.145	–0.090	–0.135	–	–0.055***	–0.010
Leadership	0.112	0.155	0.090	–	–0.043***	0.022††
Popularity	0.068	0.137	0.033	–	–0.069***	0.035†††
Understanding	–0.047	–0.009	–0.046	–	–0.038***	–0.001
Rich	0.224	0.206	0.156	?	0.018**	0.069***

***, ** Signify statistical significance at the 0.01 and 0.05 levels, respectively.
†††, †† Signify statistical significance at the 0.01 and 0.05 levels, respectively, but with a sign that is opposite predictions.
For directional predictions, significance levels are one-tailed, and for non-directional predictions, they are two-tailed. Data summarized in this table come from the HERI Freshman database, a collection of survey data describing incoming college freshmen. Columns labeled "Accounting," "Business," and "Non-Business" show changes in the mean values for each student-quality measure over time for the indicated subpopulation. The column labeled "Accounting vs. Business" shows the value in the "Accounting" column less the value in the "Business" column, while the column labeled "Accounting vs. Non-Business" shows the value in the "Accounting" column less the value in the "Non-Business" column. Statistical significance is calculated using survey-weighted Wald tests.

Variable Definitions:
H.S. GPA = high school grade-point average;
Interest = proportion of students rating "learning more about things that interest me" as a "very important" motivation for attending college;
Authority = proportion of students rating "becoming an authority in my field" as a "very important" or "essential" goal;
Leadership, Popularity, and *Understanding* = proportions of students rating themselves as above average relative to other people their age along each of these personal characteristics; and
Rich = proportion of students rating "being very well off financially" as a "very important" or "essential" goal.

when compared to non-business students, with many measures showing improvement, especially in Tables 3 and 4, where demographics and parental education are held constant. This is inconsistent with H1.

The greatest advantage of the HERI freshman data is that they are available over a longer period of time than any comparable dataset of which I am aware. However, a significant limitation of the analyses in this section is that they examine the quality of college students very early in their careers, when they express an interest in a given major, but have not yet been admitted to the major program or received significant instruction in the major. If students frequently change majors, then the results in this section may not be informative about the quality of students that attend accounting classes or graduate with accounting degrees. HERI offers a second dataset describing college seniors that is available from 1994–1999 and is linked with the freshman surveys such that individuals can be identified when they are freshmen and when they are seniors. Using this dataset, I can examine the rate at which students switched majors between their freshman and senior years of college, and whether high-quality students leave accounting degree programs at relatively high rates. I calculate the percentage of students who planned to pursue a given degree as freshmen who are still pursuing that degree as seniors, a value that I call the degree's retention rate. The retention

rate for accounting is 58 percent, which ranks in the 91st percentile and is the seventh highest retention rate of the 82 HERI degree programs. To examine whether there are unusual changes in the quality of students who express interest in accounting as freshmen versus students receiving an accounting degree as seniors, I estimate untabulated regressions in which the dependent variables are the percentile ranks of the changes in the quality of students pursuing a given degree between the freshman and senior surveys. Observations are degree/years. Independent variables are a dummy variable equal to 1 for accounting, and 0 otherwise, as well as controls for parental education, gender, race, and degree size. I find that the coefficient on the accounting dummy variable is statistically insignificant for six of the seven student-quality measures, suggesting that the quality of accounting students did not change more or less than the quality of students in other degree programs. The coefficient on the accounting dummy variable was statistically significantly positive for the change in GPA, suggesting that accounting degree programs retain students with high academic ability as measured by high school GPA.[21] Together, these results suggest that the findings for college freshmen likely hold among students graduating with accounting degrees.

The results in this section suggest that the quality of accounting education did not decline relative to the quality of a typical college education, but that it did decline relative to the quality of non-accounting business education, at least in terms of the attractiveness of these degree programs to high-quality students.

IV. INCOMES OF ACCOUNTING GRADUATES

In this section, I describe tests of H2, that the incomes of workers with accounting education declined between the mid-1960s and 2000s relative to the incomes of workers with non-accounting business education and non-business education.

IPUMS-USA Data and Models

IPUMS-USA is a collection of "microdata" describing unique individuals from the U.S. Decennial Census and, in recent years, from the Census Bureau's American Community Surveys of 2000–2011. It is constructed by randomly sampling the original census microdata from printed pages or microfilm reels and recording it in machine-readable format. In most years, the result is an unweighted or "flat" sample, but in some cases, the original sampling methodology made it impossible for IPUMS to construct flat samples. For this reason, all of my analyses use methods that account for sampling weights. Specifically, all means are calculated after weighting observations by their survey sampling probability weights, and all regressions similarly weight observations using their survey sampling probability weights using Stata's survey estimation procedures. The census datasets from which IPUMS-USA is constructed use inconsistent variable definitions that complicate the study of change over time using census data. A significant advantage of the IPUMS-USA database is that variables have been carefully harmonized across years to facilitate the study of changes over time. IPUMS-USA describes a large sample of individuals in the U.S.A. in terms of their education, work, family relationships, household characteristics, and demographics for nearly every decade since 1850.[22]

I examine the annual incomes of individuals in IPUMS-USA using variants of the following model, which I adapt from Madsen (2013):

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[21] The HERI Senior database does not include a variable describing survey sampling probability weights. As a consequence, these analyses use unweighted methods.

[22] For more detail describing the dataset or to access the data, see: https://usa.ipums.org/usa/index.shtml

$$\begin{aligned} Income_{it} = \alpha &+ \beta_1 {*} Acct_{it} + \beta_2 {*} Hours41t48_{it} + \beta_3 {*} Hours49_{it} + \beta_4 {*} Metro_{it} + \beta_5 {*} Age_{it} \\ &+ \beta_6 {*} Black_{it} + \beta_7 {*} Hispanic_{it} + \beta_8 {*} Asian_{it} + \beta_9 {*} AmerIndian_{it} + \beta_{10} {*} MiscRace_{it} \\ &+ \beta_{11} {*} Female_{it} + \beta_{12} {*} Married_{it} + \beta_{13} {*} WSD_{it} + \beta_{14} {*} Child5_{it} + \beta_{15} {*} ChildOlder_{it} \\ &+ \beta_{16} {*} Female_{it} {*} Married_{it} + \beta_{17} {*} Female_{it} {*} WSD_{it} + \beta_{18} {*} Female_{it} {*} Child5_{it} \\ &+ \beta_{19} {*} Female_{it} {*} ChildOlder_{it} + \varepsilon, \end{aligned} \tag{1}$$

where:

$Income_{it}$ = the annual income of worker i in year t in thousands of 2010 dollars (using the IPUMS-USA variable "inctot");

$Acct_{it}$ = a dummy variable equal to 1 if worker i in year t works in accounting (using IPUMS-USA variable "occ1990") and likely has an accounting education, and 0 otherwise;

$Hours41t48_{it}$ = a dummy variable equal to 1 if worker i in year t works between 41 and 48 hours in a typical work week, and 0 otherwise (using the IPUMS-USA variables "hrswork" and "uhrswork");

$Hours49_{it}$ = a dummy variable equal to 1 if worker i in year t works 49 or more hours in a typical work week, and 0 otherwise (using the IPUMS-USA variables "hrswork" and "uhrswork");

$Metro_{it}$ = a dummy variable equal to 1 if worker i in year t lived in a metropolitan area, as defined by the Office of Management and Budget, and 0 otherwise (using the IPUMS-USA variable "metro");

Age_{it} = the age in years of worker i in year t (using the IPUMS-USA variable "age");

$Black_{it}$ = a dummy variable equal to 1 if worker i in year t was classified as "black/negro," and 0 otherwise (all race categorizations use the IPUMS-USA variable "race");

$Hispanic_{it}$ = a dummy variable equal to 1 if worker i in year t was classified as Hispanic, and 0 otherwise (using the IPUMS-USA variable "hispan");

$Asian_{it}$ = a dummy variable equal to 1 if worker i in year t was classified as Asian or Pacific Islander, and 0 otherwise;

$AmerIndian_{it}$ = a dummy variable equal to 1 if worker i in year t was classified as American Indian or Alaskan Native, and 0 otherwise;

$MiscRace_{it}$ = a dummy variable equal to 1 if worker i in year t was classified as multi-racial or a non-white race that is not elsewhere classified, and 0 otherwise;

$Female_{it}$ = a dummy variable equal to 1 if worker i in year t was female, and 0 otherwise (using the IPUMS-USA variable "sex");

$Married_{it}$ = a dummy variable equal to 1 if worker i in year t was married and 0 otherwise (using the IPUMS-USA variable "marst");

WSD_{it} = a dummy variable equal to 1 if worker i in year t had been widowed, separated, or divorced, and 0 otherwise (using the IPUMS-USA variable "marst");

$Child5_{it}$ = a count of the number of worker i's own children under the age of five living with worker i in year t (using the IPUMS-USA variable "nchlt5"); and

$ChildOlder_{it}$ = a count of the number of worker i's own children over the age of four living with worker i in year t (using the IPUMS-USA variable "nchild").

I estimate this model using survey-weighted ordinary least squares (OLS) on samples of full-time (35 hours per week or more), college-educated workers 30 years old and younger. Estimates of β_1 can be interpreted as the difference between the incomes of workers with accounting education and the incomes of workers with other types of education in the sample, which I call relative income for accountants. Positive β_1 coefficients signify that workers with accounting education earn more than workers with non-accounting education at a given time; negative β_1 coefficients signify that they earn less. My tests involve comparing β_1 coefficients estimated on data from 1970 to coefficients estimated

on data from 2000 and 2010. Finding that estimated β_1 coefficients became smaller over time would be consistent with the claim that the quality of accounting education declined over the period. Control variables include measured worker characteristics that I expect could be associated with the choice of field of education and that have been shown in prior research to explain variation in incomes. I interact the *Female* dummy variable with a number of lifestyle variables because prior research has shown that they have different associations with the incomes of men and women (Farrell 2005; Madsen 2013).

I identify workers with accounting education in the IPUMS sample using the census occupation code representing "accountants and auditors," along with education values. It is not possible to test with IPUMS data whether workers in the "accountants and auditors" occupation actually received an accounting education. I use NSCG data to examine the field of college degrees of workers in the NSCG's "accountants, auditors, and other financial specialists" occupation category in 1993. Forty-one percent of workers in the NSCG's accounting occupation have accounting degrees, compared with only 5 percent of workers in other occupations.[23] This analysis confirms that the "accountants and auditors" IPUMS occupation likely contains a distinctively high concentration of workers who received an accounting education. Other NSCG occupations with relatively high concentrations of accounting degree holders include "accounting clerks and bookkeepers" (25 percent) and "top level managers, executives, managers" (13 percent).

In addition to the quality of their education, a number of factors are likely of first-order importance in explaining variation in worker incomes. These include some personal characteristics that I cannot measure, like the years and nature of prior work experience, and some macroeconomic variables that I cannot measure, like the aggregate demand for accounting services. In order to reduce the confounding impact of unmeasured personal characteristics, my tests examine only workers under 31 years old, whose incomes I expect to most directly reflect the economic value of the knowledge they acquired in college. In addition, I restrict the sample to include only workers reporting four years of college education or five or more years of college education (the highest level of college attainment consistently described in IPUMS-USA). Some of my tests examine the incomes of accountants relative to workers in any occupation. But in order to reduce the confounding impact of unmeasured macroeconomic variables, many of my tests also examine the incomes of accountants relative to workers in only other business occupations.[24] While this procedure does not guarantee that the results are unaffected by macroeconomic changes, it does hold constant macroeconomic changes that are general to occupations with labor market roles similar to accounting.

Table 5 shows mean values for the variables in Model (1), as well as the percentages reporting four years of college education and five or more years of college education, calculated for full-time workers under 31 years old in accounting, in non-accounting business occupations, and in non-business occupations in 1970, 2000, and 2010. Without considering controls, the incomes of accountants have been quite stable over the sample period, while the incomes of workers in other business occupations and non-business occupations increased between 1970 and 2000 but then dropped again by 2010 to near their 1970 levels.[25] Accountants and workers in non-accounting

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[23] These analyses involve comparing the proportions of workers in various occupations holding various degrees. When calculating these proportions, observations are weighted to account for survey sampling probabilities, which are given in the NSCG by the "wtsurvy" variable.

[24] I consider IPUMS-USA occupations to be business occupations if their value for the "occ1990" variable is between 2 and 38. These codes include workers in the "executive, administrative, and managerial occupations" and the "management related occupations." For more details, see: https://usa.ipums.org/usa/index.shtml

[25] To evaluate the comparability of my data to those used in some prior research, I compare my salary estimates against salary estimates reported in AS (2000, Chapter 2). AS report that, according to the National Association of Colleges and Employers (NACE), graduates with bachelor's degrees working in entry-level public and private accounting jobs earned $34,500, on average, in 1999, equivalent to $45,155 in 2010 dollars. My data show that four-year degree holders 30 years old and younger working in the IPUMS-USA accounting occupation in 2000 earned $48,316 in 2010 dollars. A somewhat higher number should be expected from my data given that my workers are not all entry-level workers and likely received some pay raises during their first several years on the job.

TABLE 5

Descriptive Statistics for Full-Time Workers in IPUMS-USA Under 31 Years Old with At Least Four Years of College Education Working in Accounting, Non-Accounting Business Occupations, and Non-Business Occupations in 1970, 2000, and 2010

	1970			2000			2010		
	Accounting	Business	Non-Business	Accounting	Business	Non-Business	Accounting	Business	Non-Business
Income (annual, in thousands of 2010 $s)	50.80	50.04	38.50	50.13	55.34	41.10	50.77	51.14	37.28
4 Years College	0.80	0.65	0.65	0.86	0.84	0.80	0.79	0.82	0.78
5+ Years College	0.20	0.35	0.35	0.14	0.16	0.20	0.21	0.18	0.22
Hours41t48	0.20	0.19	0.12	0.20	0.17	0.12	0.17	0.14	0.09
Hours49	0.22	0.20	0.10	0.24	0.34	0.21	0.21	0.27	0.16
Metro	0.82	0.76	0.72	0.78	0.79	0.70	0.92	0.91	0.86
Age	25.95	26.34	25.65	26.59	26.98	26.60	26.53	26.93	26.41
Black	0.02	0.04	0.04	0.08	0.06	0.07	0.06	0.07	0.08
Hispanic	0.01	0.01	0.01	0.05	0.05	0.06	0.08	0.08	0.08
Asian	0.02	0.02	0.02	0.10	0.07	0.08	0.15	0.09	0.09
AmerIndian	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
MiscRace	0.00	0.00	0.00	0.04	0.04	0.04	0.04	0.04	0.05
Female	0.09	0.20	0.40	0.56	0.52	0.54	0.55	0.53	0.57
Married	0.70	0.66	0.63	0.43	0.42	0.40	0.37	0.35	0.33
WSD	0.03	0.05	0.03	0.03	0.03	0.03	0.02	0.02	0.02
Child5	0.58	0.52	0.36	0.18	0.19	0.19	0.18	0.18	0.17
ChildOlder	0.16	0.19	0.12	0.05	0.05	0.06	0.06	0.05	0.05
*Female * Married*	0.05	0.09	0.22	0.26	0.21	0.22	0.22	0.18	0.20
*Female * WSD*	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
*Female * Child5*	0.03	0.04	0.07	0.11	0.08	0.11	0.11	0.08	0.11
*Female * ChildOlder*	0.01	0.02	0.04	0.04	0.03	0.04	0.05	0.03	0.04

Data summarized in this table come from the IPUMS-USA database. Values are means for the indicated subsamples. Columns labeled "Accounting" show values for individuals working full-time in the "Accountants and Auditors" occupation. Columns labeled "Business" show values for individuals working full-time in non-accounting business occupations (IPUMS-USA "OCC1990" codes greater than 2 and less than 38). Columns labeled "Non-Business" show values for individuals working full-time in non-business occupations.

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TABLE 5 (continued)

Variable Definitions:
Income = total annual incomes of individuals in thousands of 2010 dollars;
Hours41t48 = a dummy variable equal to 1 if the individual works, on average, between 41 and 48 hours a week, and 0 otherwise;
Hours49 = a dummy variable equal to 1 if the individual works, on average, 49 or more hours a week, and 0 otherwise;
Metro = a dummy variable equal to 1 for workers living in an area defined by the Office of Management and Budget as a metropolitan area, and 0 otherwise;
Age = individuals' ages;
Black, Hispanic, Asian, AmerIndian, and MiscRace = dummy variables equal to 1 for individuals in each race/ethnicity category, and 0 otherwise;
Female = a dummy variable equal to 1 for females, and 0 otherwise;
Married and *WSD* = dummy variables equal to 1 for individuals with the applicable relationship status (married, widowed, separated, or divorced), and 0 otherwise;
Child5= a count of the number of each individual's own children younger than five years old living with them; and
ChildOlder = a count of the number of each individual's own children five years old or older living with them.

business occupations work longer hours than workers in non-business occupations. Accountants are also more likely to be married and Asian than other workers.

IPUMS-USA Results

Table 6 shows univariate correlations among the variables in Model (1), calculated by pooling the data from 1970, 2000, and 2010, with observations weighted to provide population-level correlation estimates.[26] It shows that income is significantly positively correlated with the *accounting* dummy, five or more years of college education, work hours, age, *married,* and numbers of children. Income is significantly negatively correlated with several of the race variables. *Acct* is significantly positively correlated with longer work hours, metro location, and *Asian.*

To test H2, I estimate β_1 coefficients using Model (1) in 1970, 2000, and 2010, and in four subsamples of full-time workers under the age of 31. I include analyses of both 2000 and 2010 because the Sarbanes-Oxley Act of 2002 may have positively influenced labor market conditions for accountants. By comparing results from 2000 to 2010, I can get a better idea about whether the 2010 results were affected by SOX and other developments in the 2000s. Table 7 shows regression results for one of my subsamples, full-time workers under 31 with four years of college education working in business occupations, estimated in the years 1970, 2000, and 2010. I report the full regression results, including controls, for only these models for parsimony and because variation in the coefficients on the control variables is not the focus of this study. The models explain between 16 percent and 28 percent of the variation in annual income. The coefficients on work hours measures are positive and increasing, suggesting that the returns to overtime work have increased. Having young children is positively associated with income for males in 1970 and 2000, but when this variable is interacted with the female dummy variable, the coefficient is large and negative, consistent with young children reducing the incomes of women. Estimated coefficients on the *accounting* dummy variable, β_1, are statistically insignificant in 1970 and 2000 and significantly positive in 2010. The 1.86 coefficient on the *accounting* dummy variable in 2010 can be interpreted to mean that, in the sample of business workers under 31 in 2010, accountants earned $1,860 more than similar counterparts in other occupations.

Table 8 shows β_1 coefficients from Model (1) and changes in them over time for my subsamples of full-time workers under 31. The statistical significance of changes in β_1 coefficients estimated at different times displayed in Table 8 are estimated by running versions of Model (1) that include the independent variables listed in Model (1), a dummy variable equal to 1 for observations from the later time period, and 0 otherwise, and interactions of each independent variable with the time dummy variable. These regressions are estimated on data from the two time periods together. The magnitude and statistical significance of the change is given by the coefficient on the interaction of the accounting dummy with the time dummy. The subsamples I examine are workers in business occupations with four years of college education, workers in business occupations with five or more years of education, workers in non-business occupations with four years of college education, and workers in non-business occupations with five or more years of college education. Table 8 shows that the incomes of accountants were not statistically significantly different from those of workers in other business occupations in five of the six comparisons. The only significant difference was a premium for accountants of $1,860 per year in 2010. Relative to workers in non-business occupations, accountants consistently earned premiums in their annual incomes of between $5,430 and $10,910.

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[26] I use the procedure recommended by Sribney (2005) for calculating population-level correlations and significance levels with weighted survey data.

TABLE 6

Estimates of the Univariate Correlations among Model 1 Variables for Full-Time Workers under 31 Years Old in IPUMS-USA with Four or More Years of College Education

Panel A: Correlations Among Model 1 Variables *Income* to *Black*

	1	2	3	4	5	6	7
1. *Income*							
2. *Accountant*	0.08***						
3. *Hours41t48*	0.15***	0.04***					
4. *Hours49*	0.23***	0.02***	−0.17***				
5. *Metro*	0.08***	0.03***	0.00	0.02***			
6. *Age*	0.40***	0.00	0.06***	0.13***	0.01***		
7. *Black*	−0.04***	0.00	−0.03***	−0.04***	0.04***	0.01***	
8. *Hispanic*	−0.04***	0.00	−0.02***	−0.02***	0.07***	0.01***	−0.04***
9. *Asian*	0.01***	0.03***	−0.03***	−0.01***	0.09***	0.04***	−0.08***
10. *AmerIndian*	−0.01***	0.00	0.00	−0.01**	−0.02***	0.00	−0.01***
11. *MiscRace*	−0.04***	0.00	−0.02***	−0.01***	0.05***	0.00	−0.05***
12. *Female*	−0.17***	−0.01***	−0.05***	−0.13***	0.01*	−0.06***	0.05***
13. *Married*	0.21***	0.01***	0.04***	0.02***	−0.12***	0.27***	−0.07***
14. *WSD*	0.03***	0.00	0.00	0.01*	−0.01***	0.09***	0.02***
15. *Child5*	0.12***	0.01***	0.02***	0.01***	−0.10***	0.24***	0.01**
16. *ChildOlder*	0.05***	0.00	0.00	0.00	−0.06***	0.17***	0.06***

Panel B: Correlations Among Model 1 Variables *Hispanic* to *ChildOlder*

	8	9	10	11	12	13	14	15
8. *Hispanic*								
9. *Asian*	−0.07***							
10. *AmerIndian*	0.02***	−0.02***						
11. *MiscRace*	0.39***	−0.06***	−0.01***					
12. *Female*	0.02***	−0.02***	0.00	0.02***				
13. *Married*	−0.04***	−0.04***	0.00	−0.03***	−0.03***			
14. *WSD*	0.02***	−0.02***	0.00	0.01***	0.05***	−0.14***		
15. *Child5*	0.00	−0.05***	0.00	−0.01***	−0.06***	0.43***	−0.01***	
16. *ChildOlder*	0.03***	−0.04***	0.01***	0.01***	0.01**	0.17***	0.07***	0.19***

***, **, * Indicate two-tailed statistical significance at the 0.01, 0.05, and 0.1 levels, respectively.
Data summarized in this table come from the IPUMS-USA database. Coefficients are survey-weighted Pearson correlations. Spearman correlations (untabulated) are nearly identical to the reported coefficients. Significance levels are calculated using the method recommended in Sribney (2005).

Variable Definitions:
Income = total annual income in thousands of 2010 dollars;
4 Years College = a dummy variable equal to 1 for individuals that have attended four years of college, and 0 otherwise;
5+ Years College = a dummy variable equal to 1 for individuals that have attended five or more years of college and 0 otherwise;
Hours41t48 = a dummy variable equal to 1 if the individual works, on average, between 41 and 48 hours a week and 0 otherwise;
Hours49 = a dummy variable equal to 1 if the individual works, on average, 49 or more hours a week, and 0 otherwise;
Metro = a dummy variable equal to 1 for workers living in an area defined by the Census Bureau as a metropolitan area, and 0 otherwise;
Age = individuals' ages;

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TABLE 6 (continued)

Black, Hispanic, Asian, AmerIndian, and MiscRace = dummy variables equal to 1 for individuals in each race/ethnicity category, and 0 otherwise;
Female = a dummy variable equal to 1 for females, and 0 otherwise;
Married and *WSD* = dummy variables equal to 1 for individuals with the applicable relationship status (married, widowed, separated, or divorced), and 0 otherwise;
Child5= a count of the number of each individual's own children younger than five years old living with them; and
ChildOlder = a count of the number of each individual's own children five years old or older living with them.

When I examine how the incomes of accountants with five or more years of college education changed over time, I find in Table 8 that the changes from 1970 to 2000 and 1970 to 2010 were statistically indistinguishable from those for workers in non-accounting business occupations and were positive relative to workers in non-business occupations. The incomes of accountants with four years of college education significantly declined relative to workers in other business occupations from 1970 to 2000, but the change was no longer statistically significantly different over 1970 to 2010. This is consistent with a situation in which pay for some accountants lagged other business occupations in 2000, but was brought back to approximate parity with other business workers in 2010, following SOX. Finally, I find that the premium in annual income received by accountants with four years of college education relative to non-business occupations grew larger by $5,150 between 1970 and 2010.

The evidence in this section is inconsistent with a significant decline in the quality of accounting education over the sample period, especially for graduate education. Accountants' incomes improved relative to workers in non-business fields. However, the evidence suggests that accountants with bachelor's degrees may have suffered a small decline in relative annual income between 1970 and 2000 when compared against workers in other business occupations holding bachelor's degrees. The estimated magnitude in Table 8 is $2,120, or about 4.2 percent of total income, which is no longer evident when the window is extended to 2010. This is consistent with a modest increase in relative pay for accountants due to changes occurring during the 2000s, including SOX.

V. CONCLUSIONS

Since the 1960s, a steady stream of studies and commentaries by accounting academics and practitioners, as well as reports from professional accounting organizations, has argued that the quality of accounting education is declining (AAA 1986; Albrecht and Sack 2000; Baxter 1979; Demski 2007; Fellingham 2007; Mautz 1965; Sunder 2009, 2010, 2011; Weiser 1966; West 2003; Zeff 1989). While accounting graduates may have significant shortcomings, it is difficult to judge from existing evidence whether these shortcomings represent serious threats to the health of the accounting profession or whether they represent shortcomings typical of many types of college education. Existing evidence characterizes accounting students and the opinions of many members of the accounting community about accounting education quality with little comparative information from other fields. In this study, I identify indicators of accounting education quality that are observable over 40 years of history and for many comparison disciplines, and test whether accounting education is showing symptoms of declining quality. I focus specifically on several measures of the quality of students selecting into accounting degree programs and the relative incomes of young, college-educated accountants. This approach enables me to quantitatively characterize the quality of accounting education in the 1970s and 2000s and estimate how it has changed relative to other disciplines.

TABLE 7

Estimates of the Returns to Four Years of College Education for Young Full-Time Workers in Accounting Relative to Young Full-Time Workers in Non-Accounting Business Occupations

	1970	2000	2010
Acct	1.00	−1.12	1.86**
Hours41t48	3.39***	9.80***	9.15***
Hours49	8.09***	20.77***	20.83***
Metro	4.23***	10.59***	9.79***
Age	4.21***	3.68***	3.79***
Black	−3.83	−3.81***	−5.09***
Hispanic	−4.91	−0.42	−2.97**
Asian	−9.90***	3.72**	5.02***
AmerIndian	17.94***	−0.28	−2.43
MiscRace	10.87***	−5.69***	0.46
Female	−1.57	−5.85***	−6.25***
Married	5.14***	7.11***	1.00
WSD	12.36***	1.60	−5.69**
Child5	4.35***	4.29**	2.69
ChildOlder	6.49***	−1.85	−2.44
*Female * Married*	−7.18***	−4.83**	0.09
*Female * WSD*	−8.09	−1.12	7.54
*Female * Child5*	−8.78***	−8.36***	−5.05**
*Female * ChildOlder*	−3.03	1.66	−0.22
Constant	−71.89***	−60.80***	−64.67***
n-unweighted	3,066	10,529	11,805
n-weighted	306,600	1,218,428	1,299,699
R^2	28%	16%	16%

***, ** Indicate two-tailed statistical significance at the 0.01 and 0.05 levels, respectively.

This table shows estimates of Model (1) calculated using survey-weighted OLS and data from IPUMS-USA. The samples include full-time workers in business disciplines reporting four years of college education. n-unweighted is the number of observations in the applicable sample. n-weighted is an estimate of the total number of individuals represented by observations in the applicable sample.

Variable Definitions:

Income = a dependent variable equal to an annual value in thousands of 2010 dollars;
Acct = a dummy variable equal to 1 for people in the "Accountants and Auditors" occupation, and 0 otherwise;
Hours41t48 = a dummy variable equal to 1 if the individual works, on average, between 41 and 48 hours a week, and 0 otherwise;
Hours49 = a dummy variable equal to 1 if the individual works, on average, 49 or more hours a week, and 0 otherwise;
Metro = a dummy variable equal to 1 for workers living in an area defined by the Office of Management and Budget as a metropolitan area, and 0 otherwise;
Age = individuals' ages;
Black, Hispanic, Asian, AmerIndian, and *MiscRace* = dummy variables equal to 1 for individuals in each race/ethnicity category, and 0 otherwise;
Female = a dummy variable equal to 1 for females, and 0 otherwise;
Married and *WSD* = dummy variables equal to 1 for individuals with the applicable relationship status (married, widowed, separated, or divorced), and 0 otherwise;
Child5= a count of the number of each individual's own children less than five years old living with them; and
ChildOlder = a count of the number of each individual's own children five years old or older living with them.

TABLE 8

Model (1) Estimates of the Returns to Accounting Education for Full-Time Accountants less than 31 Years Old Relative to Other Workers

Panel A: Estimates of the Relative Returns to Accounting Education

	1970	2000	2010	Change 2000–1970	Change 2010–1970
Business occupations, 4 years college	1.00	−1.12	1.86**	−2.12*	0.86
Business occupations, 5+ years college	1.57	−1.65	1.44	−3.22	−0.13
Non-business occupations, 4 years college	6.34***	6.31***	10.91***	−0.02	4.58***
Non-business occupations, 5+ years college	5.43***	10.61***	10.58***	5.18*	5.15**

Panel B: Observation Counts

	1970		2000		2010	
	n	n-Weighted	n	n-Weighted	n	n-Weighted
Accountants and auditors, 4 years college	806	80,600	2,213	253,585	2,471	265,269
Accountants and auditors, 5+ years college	200	20,000	351	40,867	704	70,266
Business occupations, 4 years college	2,260	226,000	8,316	964,846	9,334	1,034,430
Business occupations, 5+ years college	1,194	119,400	1,537	180,686	2,208	229,300
Non-business occupations, 4 years college	17,664	1,766,400	40,070	4,485,580	50,446	5,534,797
Non-business occupations, 5+ years college	9,630	963,000	9,964	1,122,495	14,773	1,533,750

Results in this table are estimated using IPUMS-USA. Panel A shows estimates of β_1 from Model (1) that represent the returns to accounting education relative to the returns to other types of education in a given sample. I estimate Model (1) using samples collected at three different times—1970, 2000, and 2010—with results for each time arranged in Table 8's columns. For each time, I estimate Model (1) using one of four different subsamples, all of which contain IPUMS-USA full-time workers under 31 years old. Samples are (1) workers in business occupations with four years of college education, (2) workers in business occupations with five or more years of college education, (3) workers in non-business occupations with four years of college education, and (4) workers in non-business occupations with five or more years of college education. The final two columns in Panel A show the magnitude and statistical significance of changes in the relative returns to accounting education from 1970 to 2000, and from 1970 to 2010. Panel B shows, for each subsample in Panel A, the number of unweighted observations in columns labeled "n," and the number of observations weighted by sampling probability weights in columns labeled "n-Weighted."

I find that accounting degree programs have attracted students of remarkably consistent quality from the pool of all college students. These students have had average academic ability but weak soft skills. Non-accounting business programs have attracted students with relatively low, but improving, academic ability and relatively strong, and strengthening, soft skills. I find no evidence of a decline in accounting education quality when accounting education is compared against a typical college education. Instead I find that by many measures and especially when demographic and familial differences are held constant, accounting student quality and the pay of young accounting graduates have improved relative to non-business students and workers. However, given improvements in the quality of non-accounting business students, I find evidence of a decline in the quality of accounting students when they are compared against non-accounting business students. Specifically, over 40 years, changes in the proportion of high-quality students selecting into non-accounting business programs were more favorable than changes for accounting programs by between 1.8 percent and 7 percent after controlling for demographic and familial characteristics. I further find evidence that the pay of young accountants with bachelor's degrees (but not master's

degrees) fell relative to the pay of workers in non-accounting business occupations holding bachelor's degrees between 1970 and 2000. However, this decline disappeared by 2010, possibly because SOX increased the demand for accounting labor after 2002.

Prior research has documented a number of deficiencies in accounting education. My comparative evidence enables me to gauge the magnitude of the threat posed by such deficiencies and suggests that the threat is real, with accounting programs of the 2000s losing between 2 and 7 percent of the high-quality students to other business disciplines that they were able to retain in the 1970s. Declines of this magnitude, while important, do not likely signal the impending collapse of the accounting profession, which is, perhaps, surprising given the seriousness of the deficiencies in accounting education that have been documented previously. Indeed, my findings suggest that accounting education has maintained its appeal over the previous four decades relative to the majority of college majors, likely because all types of college education are imperfect. The survival of accounting education depends upon how well it performs relative to the available alternatives. While this study suggests that accounting has not been an overwhelming winner in the competition among educational alternatives, it has also not been a loser relative to most of the alternatives.

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Numerical Formats within Risk Disclosures and the Moderating Effect of Investors' Concerns about Management Discretion

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ABSTRACT: We report the results of two experiments that provide evidence that investors' risk judgments are affected by the numerical format used to describe outcomes within accounting disclosures. Consistent with prior research in psychology, investors assess higher risk in response to dollar-formatted disclosures than to equivalent percentage-formatted disclosures. Consistent with the Persuasion Knowledge Model (Friestad and Wright 1994), this effect is moderated when investors have both (1) awareness that management has discretion over format, and (2) sufficient cognitive capacity to consider its implications. Our results provide insight about the effects of current disclosure formats and suggest implications for managers who choose formats, investors who interpret formatted information, and regulators who consider whether to further prescribe the formats that are used in financial disclosures.

Keywords: *numerical format; sensitivity analysis; risk judgment; reporting discretion; persuasion knowledge model; cognitive capacity.*

I. INTRODUCTION

Managers have discretion over the numerical format they use in mandatory and voluntary disclosures. For example, pro forma information can be conveyed as percentages of prior-year performance or in absolute dollars. Likewise, reports on corporate social responsibility vary widely in the numerical format used to describe impact on the environment and community. Similar numerical format discretion applies to information discussed in annual reports

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and press releases. This paper examines whether investor judgments are affected by management's numerical format choices, and whether that effect is diminished when investors have the knowledge and cognitive resources necessary to consider that management has discretion over format.

Prior psychology and accounting research suggests that numerical formats may have a significant effect on investor judgments. Psychology research indicates that numerical format can influence people's perception of the size of an amount (Paivio 1991; Denes-Raj, Epstein, and Cole 1995; Pacini and Epstein 1999), and prior accounting research links perceptions of size with assessments of risk (Koonce, McAnally, and Mercer 2005b). In combination, these studies suggest that management's choice of numerical format in disclosures has the potential to affect investor risk assessments, but that link has not been tested previously. Further, prior numerical format research occurs in non-strategic settings. In financial settings, investors may be concerned that managers strategically alter information formats to affect investor perceptions, allowing investors to reduce their exposure to the effects of numerical format. Therefore, we draw on psychology research related to how individuals identify and cope with persuasion attempts (Friestad and Wright 1994) to consider how investor reactions to numerical formats change as investors' concern about management's strategic use of format increases. Our goal is to identify circumstances in which investors are more or less vulnerable to the effect of management's numerical format choices.

We report the results of two experiments in which M.B.A. student participants are asked to evaluate a target firm's commodity price risk. Participants are presented with a sensitivity analysis that discloses the decrease in earnings that will occur in response to an increase in the price of a key commodity used in the firm's production. Sensitivity analyses are required by various accounting standards, and also are present in voluntary disclosures (Financial Accounting Standards Board [FASB] 1990, 2000, 2003, 2004, 2011; Securities and Exchange Commission [SEC] 1997; Elliott, Jackson, and Smith 2010). We focus on sensitivity analyses related to Financial Reporting Release No. 48 (FRR 48), *Qualitative and Quantitative Risk Disclosures,* because it mandates a disclosure of risk information that maps directly into the risk assessments that we use for our dependent variable.

Study 1 tests whether numerical format affects investors' risk judgments, and whether that effect is mitigated when investors are aware that management has discretion over format. In a 2×2 between-participants design, we manipulate numerical format by disclosing a potential earnings decrease in percentage or dollar terms, holding constant the earnings information available to investors. We manipulate discretion over numerical format by varying whether investors are informed that the numerical format used in the sensitivity analysis is mandated or discretionary.

Results of Study 1 indicate that a mandated numerical format has a significant effect on investors' risk assessments, with investors believing that a firm using a percentage format faces lower risk than does a firm using a dollar format. Management discretion moderates the effect of format, such that format has less of an effect on investors' risk assessments when investors are informed that format is discretionary. Thus, investors are capable of correcting for the effects of numerical format on their judgments, but only when made aware that management has discretion over format.

Study 2 sheds light on the process underlying the discretion effect identified in Study 1. The Persuasion Knowledge Model (Friestad and Wright 1994) predicts that debiasing a persuasion attempt is an effortful process, suggesting that investors might be less able to debias the effects of numerical format in practice settings in which their cognitive resources must be shared among competing tasks. Such competing tasks abound in practice, with investors processing information from annual reports, press releases, analyst reports, and the like, while attending to interruptions from the web, media, email, colleagues, etc., that are common in most work settings (Jett and George 2003). Thus, Study 2 investigates a potentially important boundary condition to investor capability of debiasing the effects of numerical format on their judgments.

Study 2 uses a 2×2 between-participants design. As in Study 1, we manipulate numerical format between participants by disclosing the earnings decrease in percentage or dollar terms.

Unlike Study 1, we hold constant that investors are informed that management has discretion over numerical format, and manipulate whether participants are asked to perform an additional distractor task while completing their risk ratings. As in prior research (Fitzsimons and Williams 2000; Williams, Fitzsimons, and Block 2004), we use a competing task that is distracting, but that imparts no additional information that could affect participants' judgments—participants count the number of times they blink as they make their risk judgments.

Results of Study 2 indicate that, holding constant that investors are aware of management's discretion, numerical format has a greater effect when investors are distracted with a competing task. Results of debriefing questions suggest that investors possess knowledge of how management could use numerical format as a persuasion tactic, believing that managers will choose a percentage format rather than a dollar format for risk disclosures if they desire a higher stock price. Also, consistent with the Persuasion Knowledge Model, investors trust management less when investors do not face competing task demands and, therefore, have the cognitive resources necessary to access their knowledge of how numerical format could be used as a persuasion tactic. Thus, investors understand that management might strategically select percentage format to obscure performance and enhance valuations, but investors are more likely to activate and apply that knowledge when they are both aware of management discretion over format and have the cognitive capacity to consider its implications.

This research makes several contributions. We identify numerical format as a characteristic of financial disclosures that can affect investors' risk assessments, and show that dollar formats lead to higher risk assessments with respect to potential losses. Additionally, we provide evidence that investors' vulnerability to the effects of numerical format is reduced by the combination of awareness of managerial discretion and sufficient cognitive capacity to access persuasion knowledge. Thus, even if investors are aware of management's discretion over numerical format, their judgments may continue to be affected by numerical format given the lack of prominence of this information within the multitude of disclosures and competing tasks faced by investors. These findings are relevant to accounting literatures that examine investor responses to voluntary disclosure (e.g., Hirst, Koonce, and Venkataraman 2008; Elliott, Hobson, and Jackson 2011) and persuasion attempts in financial reporting and auditing contexts (e.g., Rich, Solomon, and Trotman 1997). They also contribute to the psychology literature by identifying concerns about the discloser's motives as a moderator for the effect of numerical format.

These results have practical implications for regulators, investors, and managers. Given their concern for the average investor, regulators might consider whether to require or encourage particular formats or multiple formats. Similarly, managers might consider implications of their numerical format choices on investor perceptions of risk, as well as perceptions of management persuasion tactics and trustworthiness. Finally, investors who are aware of the effects of numerical format on their judgments might choose to restate disclosures to a format that they view as preferable, perhaps altering information systems to make the format change automatically and avoiding the investor having to do so in the face of competing task demands.

In the next section, we discuss background literature and develop our hypotheses. In Sections III through VII, we describe Studies 1 and 2 and present their results. In Section VIII, we conclude by discussing implications and suggestions for future research.

II. STUDY 1: BACKGROUND AND HYPOTHESES

Numerical Format

We define numerical format as the presentation of numerical information as either an absolute number or in relative terms. For example, changes in revenues, expenses, or earnings could be

presented in dollar versus percentage terms. In Appendix A, we provide several examples of numerical formats used in various disclosures. As the examples suggest, numerical format varies across companies and even within the same annual report for different performance metrics. Aside from excerpts from Annual Reports, 10-K reports, and Corporate Responsibility Reports shown in Appendix A, companies also have discretion over numerical formats in press releases, on company websites, and in other forms of communication with the investor.

Even when all information is available to transform one format into another, prior research suggests that numerical format will systematically affect judgment. Prior studies in psychology provide evidence of ratio bias, whereby decision makers view ratios presented in terms of small numbers (e.g., 4/100) as smaller than equivalent ratios presented in terms of large numbers (e.g., 40,000,000/1,000,000,000) (Denes-Raj and Epstein 1994).[1] Additionally, Brase (2002) finds that people judge the severity of a disease to be greater when the mortality associated with the disease is presented in terms of the number of lives lost rather than the percentage of lives lost, even when the amount of information provided in each format is held constant (i.e., participants in both treatments are aware of the total number of lives at risk). Brase's (2002) findings imply that, in the context of financial reporting disclosure, investors may perceive a loss described in a disclosure to be more severe when presented in numerical terms (e.g., $40,000,000 loss, given earnings of $1,000,000,000) than when presented in percentage terms (i.e., 4 percent loss, given earnings of $1,000,000,000).

Prior accounting research suggests that risk assessments are a function of investor perceptions of size and likelihood of an outcome (Libby and Fishburn 1977; Lipe 1998). Moreover, both psychology and accounting studies link outcome size with decision maker/investor perceptions of risk (Yamagishi 1997; Koonce, Lipe, and McAnally 2005a). For example, Koonce et al. (2005a) find that as the size of a loss increases, so do investor perceptions of risk. Combining the implications from research in psychology (indicating a link between numerical format and perceived size) and accounting (indicating a link between perceived size of loss and risk perception) suggests a relationship between numerical format and risk perceptions:

H1: Dollar losses are perceived as riskier than equivalent percentage losses.

Persuasion Knowledge Model

Support for H1 would suggest that managers could use numerical format strategically to influence investors' risk judgments. However, an interesting aspect of accounting settings is that investors may be able to protect themselves from format effects if they understand the influence of format and use that understanding to debias their judgments.

Prior research suggests that people are capable of anticipating the effect of format on size perceptions when they are prompted to do so. For example, in the last of Brase's (2002) studies, he presents participants with several numerical formats and asks which format provides the clearest presentation, exaggerates the situation, and minimizes the situation. He finds that more participants

[1] Research suggests that ratio bias occurs as a result of two underlying heuristics: denominator neglect and the small numbers effect. Denominator neglect occurs because people are more comfortable thinking in terms of whole numbers than ratios and so fail to adequately consider the denominator (Callahan 1989; Offenbach, Gruen, and Caskey 1984; Reyna and Brainerd 1994). The small numbers effect suggests that people are better at interpreting small numbers because they are more concrete and easier to visualize (Paivio 1991; Pacini and Epstein 1999). Since denominator neglect leads the decision maker to view $40,000,000 as larger, while the small numbers effect leads the decision maker to view 4 percent as smaller, the two forces work in the same direction—leading the decision maker to view 4 percent as smaller than $40,000,000 (even given the knowledge that underlying earnings are $1,000,000,000). Determining which of the two processes is the dominant force behind the ratio bias is beyond the scope of our paper.

choose the percentage format as minimizing the situation and the absolute number format as exaggerating the situation. At the same time, more participants choose the absolute format as the clearest presentation of information than the percentage format.

When might people spontaneously consider the effect of format on their judgments? The Persuasion Knowledge Model (PKM; Friestad and Wright 1994) suggests that one circumstance in which that might occur is when investors suspect that a manager is intentionally manipulating format to affect their judgments. According to PKM, individuals develop knowledge about persuasion and, over time, learn to use that knowledge to cope with behaviors they perceive as persuasion attempts. Persuasion knowledge should allow investors to consider reasons for the persuasion attempt and respond accordingly, potentially concluding that they should be persuaded or, alternatively, that they should actively debias their judgment to remove the effects of the persuasion. Thus, utilizing persuasion knowledge is an effortful cognitive process that is not primed in every situation (Campbell and Kirmani 2000). Rather, various aspects of the decision context affect whether persuasion knowledge is primed and, therefore, whether effort is expended to consider its implications (Campbell and Kirmani 2000; Hamilton 2003; Williams et al. 2004; Oza, Srivastava, and Koukova 2010; Wentzel, Tomczak, and Herrmann 2010).

We posit that PKM describes investors' use of persuasion knowledge with respect to numerical formats in a financial disclosure setting. Prior research indicates that investors adjust for the impact of positive information disclosed by managers to a greater extent than negative information disclosed by managers (Rogers and Stocken 2005). Managers often have strong incentives and the discretion necessary to present the company in a positive light, and it appears that investors discount such information. Given that format is one of the factors that management has control over when disclosing company information, investors may consider that format reflects manager incentives and try to adjust for it. That is, once investors' persuasion knowledge is activated, they will perceive management's choice of numerical format as a persuasion attempt and adjust for the persuasion attempt in their risk estimates, thus reducing the numerical format effect predicted in H1. Therefore, we hypothesize a two-way interaction between numerical format and awareness of managers' discretion over format:

H2: The extent to which dollar losses are perceived as riskier than percentage losses decreases when investors perceive managers as having greater discretion to select numerical format.

III. STUDY 1: METHOD

Design and Participants

Study 1 uses a 2 × 2 design in which we manipulate between participants *numerical format* of a potential earnings loss (dollar versus percentage) and *manager discretion* (absent versus present, manipulated by varying whether numerical format is mandated versus discretionary).[2]

Two hundred and eight M.B.A. students from a major southeastern university were contacted via email to participate in the online study. Sixty-two M.B.A.s completed the study, for a response rate of 30 percent. The average participant has five years of working experience, has taken at least three accounting and three finance courses, and is 28 years old. Nineteen percent of participants are female. Elliott, Hodge, Kennedy, and Pronk (2007) suggest that M.B.A. students serve as an appropriate proxy for non-professional investors in tasks akin to that in our study. Other studies

[2] We also manipulated manager incentive to influence share price (low versus high) within participants. This manipulation was not significant and did not interact with any other independent variable. For brevity and since no inferences are affected, we average participant responses across the two incentive levels and exclude this variable from our discussion.

similar in context to our sensitivity analysis setting have also relied on M.B.A. student participants (Koonce et al. 2005a, 2005b; Elliott et al. 2010).[3]

Dependent Variable

The dependent variable is an assessment of the firm's commodity price risk on an 11-point Likert-type scale where the midpoint is labeled 0: "Moderate Risk," and the endpoints are labeled −5: "Much Lower" and +5: "Much Higher."

Independent Variables

We manipulate *numerical format* between participants by listing the outcome of a sensitivity analysis in dollar or percentage terms. While numerical format varies in many mandated and voluntary accounting disclosures, we use sensitivity analysis disclosures referenced in Financial Reporting Release No. 48 (FRR 48) to test our hypotheses. Sensitivity analysis disclosures allow investors to estimate how variation in an underlying determinant (in our experiment, a commodity price) affects an important outcome (in our experiment, earnings). Participants view the following risk disclosure:

> Precious metal commodity price changes, particularly changes in prices of platinum used in the manufacturing of Radko Inc. jewelry, would result in gains or losses in the fair value of financial instruments held by Radko Inc. Based on the outstanding balance of all financial instruments at Dec. 31, 2009, a hypothetical $2,000 per lb.* increase in platinum prices prevailing at this date, sustained for one year, would lead to a *$40,000,000 [4 percent]** decrease* in the company's earnings.[4] (* 2009 End of Year Price of Platinum = $20,000 per lb.; ** 2009 Earnings = $1,000,000,000)

We present participants with either the $40,000,000 or the 4 percent version of the outcome, depending on the format treatment to which they are assigned. The disclosure provides all participants with enough information to be able to calculate the information conveyed in the format condition to which they are not assigned.

We manipulate *manager discretion* between participants by varying whether numerical format is mandatory or discretionary. We inform participants assigned to the manager discretion *present* condition that "The Securities and Exchange Commission (SEC) does not mandate that firms use a particular format (percentage versus dollar) when disclosing the impact of commodity price risk on earnings. That is, *firms have a choice of disclosing* potential increases/decreases in earnings resulting from risk exposure *in either percentage or dollar terms*." We inform participants assigned to the manager discretion *absent* condition that "The Securities and Exchange Commission (SEC) mandates that firms use percentage [*dollar*] formatting when disclosing the impact of commodity price risk on earnings. That is, all *firms must disclose* potential increases/decreases in earnings resulting from risk exposure *in percentage [dollar] terms*." Whether the treatment indicates "dollar" versus "percentage" depends on the specific numerical format condition to which we randomly assign participants.

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[3] Participants were recruited from separate courses. Thirty-two participants received one extra-credit point and $4, while 30 participants received $10 for their participation. Responses across the two groups are spread across treatment conditions, and course is not significant as a main effect or interaction in any analysis, so results are based on the combined sample of 62 participants.

[4] Similar to Koonce et al. (2005a), we focus on "loss only" disclosures, given that FRR 48 only requires firms to disclose the potential losses associated with disclosed market risks and sets no requirements for the disclosure of potential gains resulting from the same market risks.

Procedure

Participants receive an email inviting them to take part in an accounting research study that they can access online via a provided link. The online materials are administered using Qualtrics software and prevent participants from changing their responses throughout the duration of the study once each answer has been submitted.

After signing a consent form and reading general instructions, participants view brief background information about a target firm and are instructed to assume that they have reviewed all prior financial information and estimated the firm's commodity price risk to be at "Moderate Risk."[5] On the following screen, participants are asked the first of several comprehension check questions to ensure that everyone has processed information necessary to complete the task (in this case, they confirm that the initial level of commodity price risk for the firm is "moderate"). Participants are informed prior to beginning the study that they can earn \$1 for every one of the comprehension check questions they answer correctly (for a maximum of \$6 added to their base payment).

Next, participants receive information about whether numerical format is discretionary or mandated (manipulating manager discretion) and answer a comprehension check question related to this manipulation. All participants then read a summary of relevant parts of Financial Reporting Release No. 48, *Qualitative and Quantitative Disclosures about Market Risk,* and are asked to assume that these disclosures have become mandatory for the target firm this year.[6] Then they are presented with the sensitivity analysis disclosure, which manipulates numerical format.

All participants then answer a comprehension check question requiring them to choose the magnitude of the disclosed earnings decrease, where those assigned to the dollar treatments choose between \$40,000,000 and \$45,000,000, and those assigned to the percentage treatments choose between 4 percent and 4.5 percent.[7] All participants then assess the firm's commodity price risk relative to the "moderate" level of risk they were asked to assume at the start of the study.

IV. STUDY 1: RESULTS AND DISCUSSION

Comprehension Checks

Six comprehension check questions appeared throughout Study 1. The questions assessed and reinforced participants' comprehension of the initial risk rating they were supposed to assume and the manipulations to which they had been assigned. Each incorrect response to the six questions was followed by a reiteration of the correct information, so we include all participants in our analyses. A total of 60 participants (97 percent) answered all comprehension check questions correctly.

Hypotheses Tests

Table 1 presents descriptive data, an ANOVA, a planned contrast used to test H2, and simple effects results.[8] Participants provided risk ratings on a Likert-type scale ranging from −5

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[5] Setting all participants' initial judgments at "Moderate Risk" and assessing change from an initial moderate level is designed to reduce noise in the dependent measure and, therefore, increase statistical power.

[6] We made this design choice to assure that participants believe the risk disclosure has not yet been incorporated in their initial estimates of the firm's commodity price risk.

[7] The comprehension check for the format manipulation did not require participants to identify the numerical format to which they were assigned among alternative formats, as participants in the mandatory format condition were not to be aware of the alternative format. Instead, to emphasize format unobtrusively, the question asked participants to select the outcome that occurred in the sensitivity analysis disclosure that was presented to them (i.e., \$40,000,000 versus \$45,000,000 or 4 percent versus 4.5 percent, depending on condition).

[8] Results are qualitatively similar if based on nonparametric analyses using a rank-transformation of the dependent variable.

TABLE 1

Study 1
Risk Ratings and Analyses

Panel A: Descriptive Statistics for Risk Ratings—Mean [Median] (Standard Deviation)

	Mandatory (No Discretion)	Discretionary (High Discretion)	Total
%	5.7 [6.0] (1.3) n = 16	6.3 [6.0] (1.4) n = 15	6.0 [6.0] (1.4) n = 31
$	7.7 [8.3] (2.0) n = 16	6.5 [6.0] (1.9) n = 15	7.1 [7.5] (2.0) n = 31
Total	6.7 [6.8] (2.0) n = 32	6.4 [6.0] (1.7) n = 30	

Panel B: Analysis of Variance for Risk Ratings

Source	df	Seq. SS	Adj. SS	Adj. MS	F	p
Discretion	1	1.433	1.433	1.433	0.50	0.483
Format (H1)	1	20.327	19.308	19.308	6.73	0.012
Discretion * Format	1	12.082	12.082	12.082	4.21	0.045
Error	58	166.308	166.308	2.867		
Total	61	200.149				

Panel C: Planned Contrasts with Weights of −2, +2, −1, +1

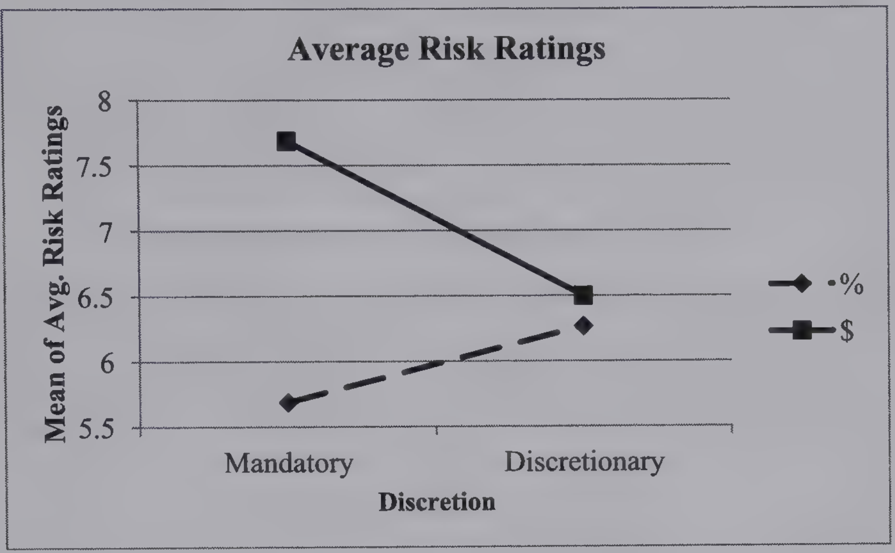
	Responses	
Source of Variation	F-statistic	p-value (two-tailed)
H2: Planned Contrast	9.49	0.003
Residual	1.155	0.322

Panel D: Simple Effects

	Risk Rating
Mandatory Format (No Discretion)	$\% \leq \$$ One-tailed p-value = 0.001
Discretionary Format (High Discretion)	$\% \neq \$$ Two-tailed p-value = 0.707
Percentage Format	Mandatory < Discretionary One-tailed p-value = 0.128
Dollar Format	Mandatory > Discretionary One-tailed p-value = 0.050

Table 1 presents Study 1's results. Panel A shows descriptive statistics, and Panel B provides the results of a standard ANOVA with Risk Rating as the dependent variable and Discretion and Format as independent variables. Panel C presents the results of a planned contrast with the following weights: −2 Mandatory/%, +2 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$. Panel D provides results of simple effects tests.

FIGURE 1
Study 1 Risk Results



This figure presents Study 1 results, collapsed across levels of incentive. Risk Ratings are the dependent variable, while Format (percentage, dollar) and Discretion (format mandatory, format up to management discretion) are the independent variables.

to +5, but to simplify interpretation of results, we recoded each set of risk ratings to a scale ranging from 1 to 11 (11 being the highest risk). Unless noted otherwise, all reported p-values are two-tailed.

H1 predicts that investors perceive losses as indicating higher risk under a dollar format than under a percentage format. Panel B of Table 1 shows that the main effect of format is significant ($p = 0.012$), supporting H1. Panel D of Table 1 provides a cleaner test of H1 by showing the simple effect of numerical format when participants view format as mandatory and, therefore, do not consider potential effects of management discretion, and again supports H1 ($p = 0.001$).

H2 predicts that the extent to which investors perceive dollar losses as riskier than percentage losses decreases when investors perceive managers as having greater discretion to select numerical format. Given the disordinal shape of our predicted interaction, we test H2 using a planned contrast with the following weights: −2 Mandatory/%, +2 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$. Panel C of Table 1 indicates that this contrast is significant ($p = 0.003$).[9] Figure 1 graphs the interaction. Simple effects tests in Panel D of Table 1 indicate that numerical format has a significant effect when mandatory (one-tailed $p = 0.001$), but not when discretionary ($p = 0.707$). As predicted, the effect of numerical format is moderated as investor knowledge of manager discretion increases. Relative to investors in the low-discretion (mandatory format) case, investors in the high-discretion case perceive significantly less risk under the dollar format (one-tailed $p = 0.050$) and (insignificantly) more risk under the percentage format (one-tailed $p = 0.128$). Overall, these results support H2.

[9] For robustness, we also test alternative contrast codes. Inferences remain the same. A contrast code with weights −3 Mandatory/%, +3 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$ is significant at $p = 0.002$. Similarly, a contrast code with weights −4 Mandatory/%, +4 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$ is significant at $p = 0.002$.

Discussion

Study 1 provides evidence that investors perceive potential earnings decreases as riskier when disclosures are formatted as dollars than when they are formatted as percentages. Further, this format effect decreases when investors are aware that management has discretion over format. These results are consistent with PKM, suggesting that increased concern over manager discretion activates knowledge about persuasion techniques and encourages investors to reduce the impact of the numerical format effect.

V. STUDY 2: BACKGROUND AND HYPOTHESIS

Study 2 builds upon Study 1 to provide a more direct test of PKM. Holding constant that all participants are informed that management has discretion over numerical format, we manipulate whether participants are distracted by a simultaneous task that imposes additional cognitive demands. Distractions in practice include other information within the same report, other related sources of company data that investors access as part of their valuation, and competing tasks such as responding to email, processing information on the web, and interacting with colleagues (Jett and George 2003). Growing research on task-switching provides evidence that navigating across competing tasks often leads to slower and more error-prone performance, suggesting lower cognitive resources available for any one task (e.g., see reviews by Monsell [2003] or Kiesel et al. [2010]). If, as indicated by Campbell and Kirmani (2000), diminished cognitive capacity reduces the extent to which persuasion knowledge is activated, we should see investors who are aware that management has discretion over numerical format less able to debias the effect of format when they also must cope with a competing distractor task. We test the following hypothesis:

H3: When investors perceive managers as having discretion over numerical format, the extent to which dollar losses are perceived as riskier than percentage losses increases when investors make risk ratings simultaneous to completing a distractor task.

VI. STUDY 2: METHOD

Design and Participants

Study 2 uses a 2 × 2 between-subjects design varying *numerical format* of a potential earnings loss (dollar versus percentage) and *distractor setting* (distractor present versus distractor absent).[10] Participants are 81 professionals with M.B.A. degrees. We obtained participants through a paid panel data service provided by Qualtrics (see: http://www.qualtrics.com/). We did not have direct contact with participants, who were compensated for completing our study by Qualtrics.[11] The

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[10] For those participants not distracted by a competing task, we also manipulated between subjects whether we specifically stated that management had discretion over numerical format (that is, we informed half of the No Distractor participants that management had discretion over numerical format, and did not inform the remaining half of the No Distractor participants about this discretion). Among those participants who were not informed about management discretion, 19 participants (49 percent of participants in that treatment condition) failed a manipulation check about whether they had been informed about management discretion over numerical format. We believe these failures occurred because participants misinterpreted the question as asking if *they were aware* that management had discretion over format, rather than if *we had informed* them of that fact. Given the large percentage of manipulation check failures, we do not include participants from the "No Distractor and Not Informed about Management Discretion" treatment in our analysis. Thus, all analyses in the "Results" section are based on conditions in which participants were informed that management had discretion over numerical format.

[11] Qualtrics does not divulge participant compensation information.

average participant has 23 years of work experience, has taken four accounting and four finance courses, and is 50 years old. Forty-seven percent of participants are female and 19 percent report having prior experience in estimating commodity price risk (stemming from either personal investments, professional experience, or both).

Dependent and Independent Variables

We operationalize the dependent variable and the numerical format independent variable as in Study 1. We manipulate Distractor by varying whether we ask participants to perform a competing task at the same time as they make their risk rating. Specifically, we ask participants in the distraction condition to count the number of times they blink as they provide their risk rating. Previous psychology research has shown this manipulation to effectively constrain cognitive capacity (Fitzsimons and Williams 2000; Williams et al. 2004). This competing task cannot be interpreted by participants as providing any information relevant to their risk ratings, but it should be sufficiently distracting to diminish the cognitive capacity available to perform risk ratings. Therefore, it should serve as an effective proxy for the various competing task demands that occupy investor attention when evaluating company information.

Procedure

After viewing background information about a target firm and assuming an initial "Moderate Risk" estimate of commodity price risk, all participants view the same risk disclosure as in Study 1. All participants receive the information provided in the Discretion Information treatment condition used in Study 1, being informed that "Management *chooses* whether to use *a percentage or dollar format* to disclose potential increases/decreases in earnings resulting from risk exposure."[12] Participants next answer a comprehension check question to assure that they are aware of management's discretion over numerical format.

Next, half of the participants move on to rate the company's commodity price risk. The other half is informed about the distractor task described above and then rate the company's commodity price risk while performing that distractor task.

At the end of the study, all participants answer several debriefing questions regarding management's competence, trustworthiness, and use of numerical formats as persuasion tactics. Participants first indicate their agreement with the following two statements: (1) "I think Radko's management has the *competence* necessary to make clear and unbiased risk disclosures," and (2) "I *trust* Radko's management to make clear and unbiased risk disclosures."[13] Participants indicate agreement on a seven-point scale ranging from "1: Strongly Disagree" to "7: Strongly Agree." Then participants provide insight about their view of how management could use numerical format for different purposes. Using an 11-point scale ranging from "–5: Definitely 4 percent" to "+5: Definitely $40,000,000," participants answer the following two questions: (1) "If management was

[12] Informing participants that management has the opportunity to strategically select disclosure format inherently includes information on the format management chose (e.g., dollar) and the format management could have chosen (e.g., percentage). In a separate, untabulated experiment, we confirmed that our findings are not the result of mere awareness of an alternative disclosure format. Specifically, we manipulated whether participants were informed about the potential for a different format, along with manipulating manager discretion and numerical format. We replicated Study 1 results (Format * Discretion interaction is significant at $p = 0.019$) and did not find a significant main or interactive effect of investor Informed status (Informed main effect and Format * Discretion * Informed interaction are not significant at $p = 0.457$ and $p = 0.615$, respectively).

[13] Competence and Trustworthiness are the two main factors comprising a source credibility scale originated by McCroskey (1966). Our questions are consistent with those asked in prior accounting credibility studies (e.g., Mercer 2005; Barton and Mercer 2005; Hirst, Koonce, and Venkataraman 2007) and validated by prior research (McCroskey 1966; Leathers 1992; Newell and Goldsmith 2001).

particularly worried about increasing Radko's stock price and therefore about investors' perceptions of firm value, which of the two numerical formats do you think it would select for the sensitivity analysis?" and (2) "If management was particularly worried about the transparency of Radko's disclosures and communicating clearly with users of their financial information, which of the two numerical formats do you think it would select for the sensitivity analysis?"

VII. STUDY 2: RESULTS AND DISCUSSION

Comprehension and Manipulation Checks

All 81 participants answered all comprehension check questions correctly. Ten (12 percent) participants did not answer the Discretion manipulation check question correctly. Excluding these participants from our analysis does not affect any inferences. All are included.

Hypothesis Tests

Table 2 presents descriptive data, an ANOVA, a planned contrast used to test H3, and simple effects results.[14] As in Study 1, to simplify interpretation of the results, we recoded each set of risk ratings to a scale ranging from 1 to 11 (11 being the highest risk). Unless noted otherwise, all reported p-values are two-tailed.

Figure 2 presents mean risk ratings in graphical form, and highlights a pattern of results consistent with H3. Participants view dollar disclosures as riskier than percentage disclosures when distracted by a competing task, despite being informed that management has discretion over numerical format. When not distracted by a competing task, participants exhibit a smaller difference in risk perceptions between dollar and percentage disclosures.

Given the disordinal nature of our prediction, we test H3 using a contrast test with the following weights: −1 No Distractor/%, +1 No Distractor/$, −2 Distractor/%, +2 Distractor/$.[15] Table 2, Panel C supports H3 with a significant contrast, indicating that distraction increases the extent to which risk ratings for dollar formats exceed those for percentage formats ($p = 0.009$).[16] Simple effects tests in Table 2, Panel D indicate that participants in the Distractor treatment provided risk ratings that differ significantly between the percentage format and dollar format conditions (one-tailed $p = 0.013$), but participants in the No Distractor treatment provided risk ratings that do not differ significantly between the percentage and dollar format conditions ($p = 0.250$).

Underlying Process Analysis

We predicted that considering management discretion over numerical format activates investors' persuasion knowledge and allows them to adjust for numerical format when making risk judgments. We conduct further analyses using debriefing data to examine investors' assessments of management's persuasion tactics, competence, and trustworthiness.

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[14] Results are qualitatively similar if based on nonparametric analyses using a rank-transformation of the dependent variable.

[15] Results are qualitatively similar if based on nonparametric analyses using a rank-transformation of the dependent variable.

[16] For robustness, we also test alternative contrast codes. Inferences remain the same. A contrast code with weights −3 Mandatory/%, +3 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$ is significant at $p < 0.01$. Similarly, a contrast code with weights −4 Mandatory/%, +4 Mandatory/$, −1 Discretionary/%, +1 Discretionary/$ is significant at $p = 0.01$.

TABLE 2

Study 2
Risk Ratings and Analyses

Panel A: Descriptive Statistics for Risk Ratings—Mean (Standard Deviation) [Median]

	No Distractor	Distractor	Total
%	6.6 [6.5] (1.4) n = 20	6.1 [6.0] (2.3) n = 17	6.3 [6.0] (1.9) n = 37
$	7.2 [7.0] (2.3) n = 24	7.6 [8.0] (1.7) n = 20	7.4 [8.0] (2.0) n = 44
Total	6.9 [7.0] (2.0) n = 44	6.9 [7.0] (2.1) n = 37	

Panel B: Analysis of Variance for Risk Ratings

Source	df	Seq. SS	Adj. SS	Adj. MS	F	p
Distractor	1	0.002	0.012	0.012	0.00	0.956
Format (H1)	1	23.653	25.240	25.240	6.47	0.013
Distractor * Format	1	4.340	4.340	4.340	1.11	0.295
Error	77	300.400	300.400	3.901		
Total	80	328.395				

Panel C: Planned Contrasts with Weights of −1, +1, −2, +2

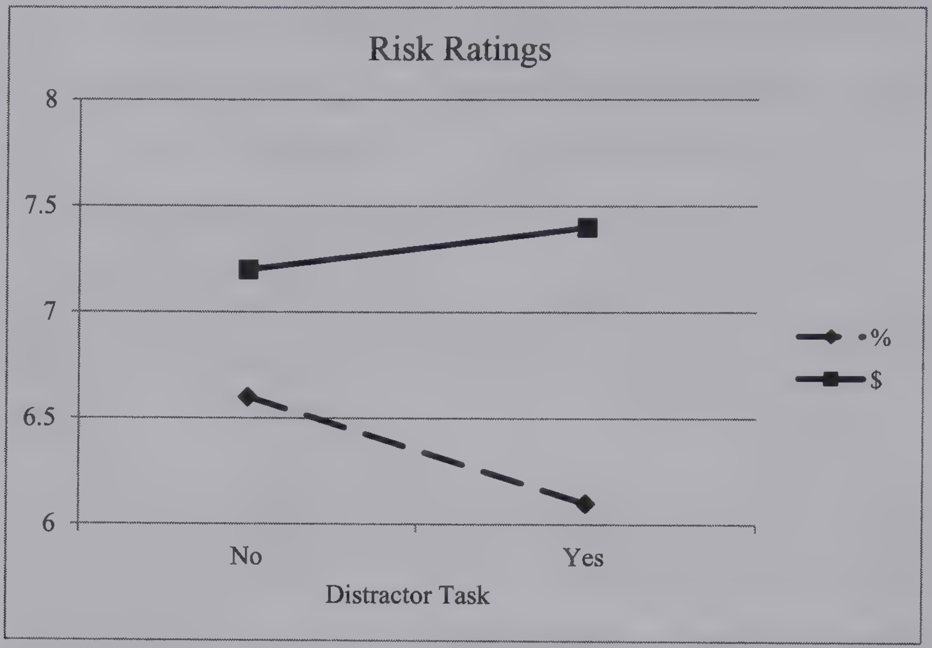
	Responses	
Source of Variation	F-statistic	p-value (two-tailed)
H3: Planned Contrast	7.17	0.009
Residual	0.001	0.999

Panel D: Simple Effects

	Risk Rating
No Distractor	% ≠ $ Two-tailed p-value = 0.250
Distractor	% < $ One-tailed p-value = 0.013
Percentage Format	No Distractor > Distractor One-tailed p-value = 0.227
Dollar Format	No Distractor < Distractor One-tailed p-value = 0.234

Table 2 presents Study 2's results. Panel A shows descriptive statistics, and Panel B provides a standard ANOVA with Risk Rating as the dependent variable and Distractor and Format as independent variables. Panel C presents the results of a planned contrast with the following weights: −1 No Distractor/%, +1 No Distractor/$, −2 Distractor/%, +2 Distractor/$. Panel D provides results of simple effects tests.

FIGURE 2
Study 2 Risk Results



This figure presents results of participants' risk ratings from Study 2. Risk Ratings are the dependent variable, and Format (percentage, dollar) and Distractor (yes, no) are the independent variables.

Management Persuasion Tactics

We assessed investors' knowledge of management persuasion tactics by asking them about management's concerns over firm value and transparency (as we describe in the "Procedure" section, above).

Participants rated management as more likely to select the percentage format to increase stock price than to increase transparency. Table 3 indicates that mean responses to the stock price question were significantly lower (indicating more preference for a percentage format) than responses to the transparency question (Panel B, $p < 0.001$), as well as significantly below the midpoint of the scale (denoted "Management Will Not Have a Preference") (Panel A, $p < 0.001$). Responses to the transparency question were not significantly different from the midpoint.[17]

Overall, these results suggest that when prompted to think about persuasion tactics, participants view dollar formats as more transparent than percentage formats, but view percentage formats as more likely to be selected by management who want to enhance share prices. If participants spontaneously considered this information, then we would expect them to reward dollar formats with lower risk ratings and to view percentage formats as indicating an attempt by management to increase valuations. Instead, the risk ratings of participants in the Distractor treatments penalize management for choosing to disclose in dollars rather than percentages. Thus, knowledge of how management might use discretion over format to affect investor decisions is necessary, but not

[17] As expected, responses to questions eliciting participants' knowledge of persuasion tactics did not differ between treatment conditions. Specifically, in analyses with either the Transparency or Stock Price ratings as the dependent variable and Format and Distractor as independent variables, format and distractor are not significant at conventional levels, either as main effects or in an interaction.

TABLE 3
Study 2
Management Persuasion Tactics When Choosing Numerical Format

Panel A: One-Sample t-test of $\mu = 0$ versus $\mu \neq 0$

Variable	n	Mean	t-statistic	p-value (two-tailed)
Stock Price	81	−2.25	−8.08	<0.001
Transparency	81	0.51	1.45	0.150

Panel B: Two-Sample t-test of Mean Stock Price Ratings $\neq$ Mean Transparency Ratings

Test	n	t-statistic	p-value (two-tailed)
$-2.25 \neq 0.51$	81	−6.18	<0.001

Panel A of Table 3 presents the results of a one-sample t-test testing whether participants' responses to each of the two questions differ significantly from the midpoint response of "Management Will Not Have a Preference." Participants responded to two questions: (1) Firm Value: "If management was particularly worried about increasing Radko's stock price and therefore about investors' perceptions of firm value, which of the two numerical formats do you think it would select for the sensitivity analysis?" and (2) Transparency: "If management was particularly worried about the transparency of Radko's disclosure and communicating clearly with users of their financial information, which of the two numerical formats do you think it would select for the sensitivity analysis?" Participants responded on an 11-point scale with −5 (Definitely 4%) and +5 (Definitely $40,000,000) as the endpoints, and 0 (Management Will Not Have a Preference) as the midpoint of the scale. Panel B presents the results of a two-sample t-test testing whether the mean responses to each of the above two questions differ significantly from one another.

sufficient—investors also must have the cognitive capacity to activate their persuasion knowledge, consider the possible strategic implications of management's choice of numerical format, and adjust for its effect.

Management Competence and Trustworthiness

Immediately after participants provided their risk assessments, we asked them to rate the extent to which Radko's management is competent and can be trusted (as we describe in the "Procedure" section, above). Untabulated analyses indicate that perceptions of management competence did not differ significantly between treatment conditions, but perceptions of management trustworthiness were higher in the distractor task condition (main effect $p = 0.055$). Given that all participants were aware that management had discretion over format, this result again suggests that investors were less likely to access that knowledge when they had reduced cognitive resources to do so.

VIII. CONCLUSION

Companies have flexibility in the numerical format they use in a variety of required and voluntary disclosures. We report results from two experiments that provide evidence that a firm's choice of numerical format can have a significant effect on investor risk judgments. Our results indicate that when management has no discretion over format, or when investors have awareness of management discretion, but lack sufficient cognitive capacity because of competing task demands, amounts formatted in dollar terms result in higher risk assessments than do equivalent amounts formatted in percentage terms. However, investors who have both the awareness of manager

discretion over format and the cognitive capacity to consider the implications of that discretion are more likely to reduce the effects of managers' format choice on their risk assessments.

Study 2's debriefing data provide evidence that, consistent with the Persuasion Knowledge Model (PKM; Friestad and Wright 1994), investors understand that management could select reporting formats to achieve particular reporting objectives, but do not access and use that knowledge unless they have to do so. Regardless of treatment condition, investors view managers as more likely to choose percentage formats to enhance valuations. However, investors who faced competing task demands during the experiment trust management to a greater extent, consistent with those investors failing to access their persuasion knowledge because of diminished cognitive resources.

Testing our predictions using an experiment provides several advantages. An experiment allows us to hold constant characteristics of the firm and the firm's disclosure that might covary with numerical format in practice. An experiment also allows us to obtain an adequate sample size of combinations of numerical format and manager discretion that are underrepresented in practice, but that shed light on the theoretical constructs we are investigating. Further, we are able to directly constrain participants' cognitive resources, yet confirm their acquisition of key information in order to test our underlying theory. Finally, we can ask participants questions to better understand the process underlying the effects we document.

Our results complement and extend the results of prior studies examining the effects of format and sensitivity analyses on investor judgments. While prior research provides evidence that investors' risk perceptions are affected by the size of numbers used in sensitivity analyses (Koonce et al. 2005b), our results indicate that the format of those numbers also affects investors' judgments. Our findings also contribute to the format effects literature in accounting. Prior studies have found that the judgments of investors and auditors are affected by the label attached to information, the location of information within financial statements, or the location of information in other voluntary disclosures (see, e.g., Hopkins 1996; Maines and McDaniel 2000; Krische 2005; Libby, Nelson, and Hunton 2006). Our study holds constant those features and instead varies the numerical format in which information is presented.

Our findings provide opportunities for future research. For example, we investigate formatting of *outcomes* in sensitivity analyses. Formatting of *determinants* (e.g., the change in commodity price that produces a change in earnings) also could affect judgment. Additionally, we focus on loss disclosures, but future research could examine these factors with respect to gain disclosures as well. From an archival perspective, our results present empirically testable hypotheses for numerical format choices in press releases, annual reports, and corporate responsibility reports. Particularly in disclosures regarding low magnitude amounts, we would expect that firms choose percentage formats when trying to minimize the perceived size of an amount, but that firms choose dollar formats when trying to maximize the perceived size of an amount.

Our findings also have implications for practice. Numerical formats vary in a variety of financial disclosures, but many investors may not spontaneously consider management discretion over format. Further, those investors who are aware of manager discretion may lack the capacity to activate their persuasion knowledge due to distraction from other tasks and considerations. Regulators could consider encouraging or mandating particular or multiple formats that they view as more in line with investor welfare, and investors could consider restating disclosures to formats that they believe are more desirable. Investors also might consider altering information systems to present information in their preferred numerical format and avoid the necessity to make adjustments when they face competing task demands. Managers may want to consider the effects that particular numerical formats produce on investor judgments of risk, as well as their assessment of management intentions and trustworthiness.

Our results are subject to several limitations. First, due to time constraints, the participants' task is a simplification of what would occur in practice, with participants assuming that they have already completed a majority of their analysis prior to encountering the sensitivity analysis disclosure that is the focus of our experiment, and that the result of that analysis was a conclusion that a moderate level of risk is present. Although this simplification affects the realism of our design, it should not affect the directional effects that we hypothesize and test. Second, the study was administered online rather than in person. Although allowing participants to access the study independently at any point during a pre-specified period of time inherently surrenders some experimental control in administering the study, the potential increased variability in responses should work against obtaining predicted results.

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APPENDIX A
Disclosure Excerpts Varying Numerical Format

Example of Variation in Numerical Format Changes Presented in Verizon's (2012) ***Annual Report***

CORPORATE HIGHLIGHTS

- $15.3 billion in free cash flow (non-GAAP)
- 4.5% growth in operating revenues
- 13.2% total shareholder return
- 3.0% annual dividend increase
- 5.9 million wireless retail connection net additions
- 0.91% wireless retail postpaid churn
- 8.4% growth in wireless retail service revenue
- 607,000 FiOS Internet subscriber net additions
- 553,000 FiOS Video subscriber net additions
- 17.2% growth in FiOS revenue
- 6.3% growth in Enterprise Strategic Services revenue

Solid lines added to highlight relative changes; dashed lines added to highlight absolute changes.

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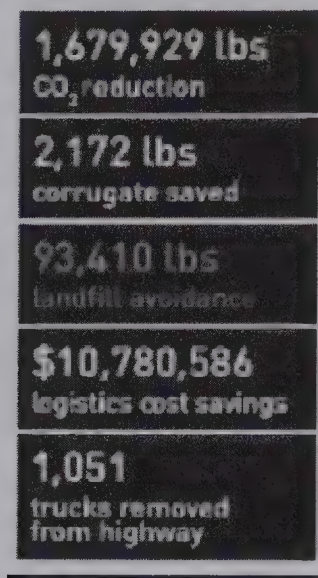
Example of Absolute Number Changes Presented in Hershey's (2012) *CSR Scorecard*



Example of Relative Number Changes Presented in Starbuck's (2012) *CSR Report*

> Conserving Energy and Water:
> In 2008, we set a goal to reduce electricity use by 25 percent in company-owned stores by 2015. Since then, we've implemented a number of energy initiatives and have seen electricity use decrease by a total of 6.5 percent. Despite these efforts, electricity use in 2012 increased 1.1 percent compared to 2011, mainly due to record-breaking temperatures in the U.S. that caused our stores' air conditioners to run more than usual.

Example of Relative Number Outcome Changes Presented in Sensitivity Analyses

> Cathay General Bancorp (2012) 10-K:
> Our simulation model also projects the net economic value of our portfolio of assets and liabilities . . . At December 31, 2012, if interest rates were to increase instantaneously by 200 basis points, the simulation indicated that the net market value of our portfolio of assets and liabilities would increase by 6.6 percent.

Example of Absolute Number Outcome Changes Presented in Sensitivity Analyses

> Coca Cola Company (2012) 10-K:
> We estimate that an unfavorable 10 percent change in rates would have increased our net losses by $372 million.

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Does SOX 404 Have Teeth? Consequences of the Failure to Report Existing Internal Control Weaknesses

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ABSTRACT: We examine various penalties that could serve as enforcement mechanisms for Sarbanes-Oxley (SOX) Section 404. We focus on firms with restatements, some of which had previously reported their control weaknesses as required and some of which acknowledged them only after announcing their restatement. We find no evidence that penalties are more likely for firms, managers, or auditors that fail to report existing control weaknesses. Instead, class action lawsuits, management turnover, and auditor turnover are all more likely in the wake of a restatement when control weaknesses had previously been reported. We find similar, although weaker, evidence for Securities and Exchange Commission (SEC) sanctions. These results are consistent with disclosure of control weaknesses making it difficult for management to plausibly claim later that they were unaware of the underlying conditions that led to restatements. The results also suggest that the public and private enforcement mechanisms surrounding SOX 404 are unlikely to provide strong incentives for compliance and offer a potential explanation for why most restatements are issued by firms that previously claimed to have effective internal controls.

Keywords: *Sarbanes-Oxley Act; internal controls; enforcement; restatements.*

I. INTRODUCTION

In this paper, we examine several potential consequences of failing to report existing control weaknesses as required by Section 404 of the Sarbanes-Oxley Act of 2002 (hereafter, SOX 404). Our investigation is motivated largely by recent concerns about the reliability of SOX

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404 reports and related evidence of firms claiming to have effective internal controls over financial reporting when they instead have material weaknesses in those controls (e.g., Turner and Weirich 2006; Glass Lewis & Co. 2007; Institute of Management Accountants [IMA] 2008; Besch 2009; Plumlee and Yohn 2010; Public Company Accounting Oversight Board [PCAOB] 2012; Rice and Weber 2012). Understanding the consequences of such reporting failures is important because it bears on managers' and auditors' incentives to detect and disclose internal control weaknesses and, thus, on the effectiveness of SOX 404 in achieving its intended goal of boosting investor confidence in the reliability of financial reports. This importance is underscored by the high costs of control audits, which have made these requirements the most controversial aspect of SOX (Coates and Srinivasan 2014).

Under SOX 404, firms and their auditors are required to provide formal opinions on the effectiveness of internal controls over financial reporting within the annual 10-K filing.[1] Within those opinions, they must state whether internal controls are effective or ineffective. Internal controls are only to be deemed effective if there are no material weaknesses, which are defined as control deficiencies that result in the likelihood of a material misstatement being more than remote (PCAOB 2004). If material weaknesses exist, then they are required to be reported, along with a description of their nature. This requirement, which was promulgated in the aftermath of several high-profile accounting scandals, is intended to enhance investor confidence in the reliability of financial reporting by providing an early warning of the possibility of impending accounting problems (PCAOB 2004; Cunningham 2004).[2]

However, concerns have begun to emerge about the reliability of SOX 404 reports, and the effectiveness of SOX 404 in providing advance warning of potential accounting problems remains unclear. For example, the SEC has suggested that the decrease in reported control weaknesses in recent years "could be due to material weaknesses not being identified or reported," as opposed to improvements in the underlying controls (Besch 2009; see, also, Whitehouse 2009, 2010). Consistent with that concern, the PCAOB recently reported that 22 percent of the internal control audits it reviewed for 2011 were deficient, as were 15 percent for 2010 (PCAOB 2012; see, also, Rapoport 2012). Practitioners have also questioned the vigor of enforcement, noting that the SEC eliminated its accounting fraud task force in a recent reorganization (e.g., McKenna 2012).[3]

Recent evidence from academic research highlights similar concerns. Rice and Weber (2012) study a sample of firms with restatements stemming from underlying control weaknesses and find that only a minority of these firms report their weaknesses prior to the related restatements. Thus, in many cases, investors are not warned that the possibility of a material misstatement in the financial reports is more than remote until *after* the need to correct such a misstatement has been announced. In addition, Plumlee and Yohn (2010) document that restatements have generally outpaced reported internal control weaknesses in recent years, and suggest that many weaknesses likely go unreported.

We extend this previous research by examining whether there are substantive consequences associated with failing to report existing internal control weaknesses as required by SOX 404. Understanding these consequences is important because the evidence from previous research

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[1] We use "SOX 404" to refer collectively to the requirements under §404(a) (management's report) and §404(b) (the auditor's report); our empirical analyses also focus on firms with both management and auditor reports. Non-accelerated filers (firms with public float less than $75 million) are exempt from §404(b) and, thus, are excluded.

[2] The Securities and Exchange Commission (SEC 2005) notes that "a central purpose of the assessment of internal control over financial reporting is to identify material weaknesses that have, by their very definition, more than a remote likelihood of leading to a material misstatement in the financial statements."

[3] For example, Jack Ciesielski, owner of research firm R. G. Associates and publisher of The Analyst's Accounting Observer, is quoted in McKenna (2012, 46) arguing, "SEC enforcement of Sarbanes-Oxley has been minimal. Sarbanes-Oxley may have brought us some peace for our time, but without vigilance through long-term enforcement, it can't last."

suggests a potential disconnect between the intended goals of SOX 404 and the enforcement mechanisms that surround its implementation.[4] In our analyses, we consider various regulatory, legal, and labor market-based penalties that could serve as potential enforcement mechanisms for SOX 404, including Accounting and Auditing Enforcement Releases (AAERs) issued by the SEC, class action lawsuits filed by investors, top management turnover, and auditor turnover. In doing so, we consider both public and private mechanisms, as well as potential penalties against the firm, its managers, and its auditor.

Our analyses focus on a sample of firms that are subject to SOX 404 and that also have restatements. This sample has two important features. First, while all of our sample firms have misstatements, many firms acknowledge the control weaknesses underlying those misstatements only after announcing the need for a restatement. Because restatements provide a mechanism to (*ex post*) identify unreported control weaknesses, our design allows us to compare the consequences for firms that reported the existence of their control weaknesses in a timely manner with those that did not. Second, consequences are unlikely for firms without restatements because their failure to report existing weaknesses may never come to light. However, within our sample, unreported control weaknesses are revealed by the restatement; thus, to the extent that consequences exist, they are likely to be concentrated among firms with restatements.[5]

Our empirical strategy can be summarized as follows. We first identify a sample of firms with restatements, which are generally indicative of control weaknesses. We then examine whether various consequences of the restatements differ between those firms that reported control weaknesses prior to the restatement and those that did not, controlling for restatement severity and other relevant factors. We focus on consequences that could serve as potential enforcement mechanisms for SOX 404 (i.e., penalties faced by firms, their managers, and their auditors for failing to detect and disclose existing control weaknesses).[6] Because our main variable of interest (whether firms report their control weaknesses prior to the restatement) is potentially endogenous, in supplementary analyses, we also use propensity score-matching and bivariate probit estimation to capture observable and unobservable factors, respectively, that may affect firms' *ex ante* propensities to report existing weaknesses.

The economics literature has long recognized the importance of enforcement for incentivizing compliance with laws, regulations, and other prescribed behavior (e.g., Becker 1968; Stigler 1970). This literature is generally based on the assumption that agents consider the expected costs and benefits in deciding whether to comply with laws. The strength of enforcement, then, affects the expected costs of noncompliance. The accounting literature has also recently begun to document the important role of enforcement in determining the usefulness of various reporting standards and in reducing misreporting. For example, Kedia and Rajgopal (2011) show that due to resource constraints, the SEC is more likely to investigate firms located closer to its offices, and that these firms are, in turn, less likely to adopt aggressive accounting practices (see Leuz and Wysocki [2009] for a survey on reporting regulation and enforcement). If enforcement is stringent, then we expect

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[4] For example, the PCAOB (2004) has argued, "For the implementation of Section 404 of the Act to achieve its objectives, the public must have confidence that all material weaknesses that exist as of the company's year-end will be publicly reported." The findings of Rice and Weber (2012) suggest that this condition is violated, as the majority of restating firms previously claimed to have effective controls. One potential explanation for these reporting failures is a lack of substantive penalties.

[5] In the absence of a restatement, the only direct public signal of control effectiveness is the control report itself. Thus, without a restatement, regulators and market participants will likely never discover the existence of unreported weaknesses or the associated reporting failures.

[6] As detailed in Section IV, we also conduct our tests on a subsample that is constrained to only those firms that explicitly acknowledge the existence of control weaknesses in the aftermath of their restatement, with generally similar results.

penalties surrounding restatements to be more likely for firms that previously claimed that their controls were effective.

However, there are also reasons to instead expect that compliance with SOX 404 through the timely acknowledgement of ineffective controls might actually *increase* the likelihood of penalties in the event of a later restatement. First, the public disclosure of control weaknesses likely brings firms to the attention of regulators and class action law firms as potential targets of investigations and litigation. These parties pursue targets where their chance of winning is highest, and the disclosure of control weaknesses is effectively a tacit admission that the firm is in violation of the internal control provisions of the Foreign Corrupt Practices Act (FCPA).[7] Second, the disclosure of control weaknesses serves to acknowledge managements' and auditors' awareness of the existence of those weaknesses, which makes it more difficult for them to plausibly claim later that they were unaware of the conditions in the control environment that led to the restatement. Therefore, whether the enforcement of SOX 404 is stringent enough to overcome these opposing forces is an empirical question.

We begin our empirical analysis with the public enforcement of SOX 404, focusing on AAERs issued by the SEC. For most specifications, we find that the likelihood of receiving an AAER following a restatement is similar regardless of whether firms had reported their control weaknesses or instead claimed that their controls were effective prior to the restatement. For our propensity score-matched sample, we find that the prior acknowledgment of control weaknesses increases the likelihood of receiving an AAER by about 6 percent. Taken together, we find no evidence of vigorous public enforcement of SOX 404; instead, the evidence is suggestive of the opposite: that reported control weaknesses aid the SEC in identifying cases where potential enforcement actions are likely to succeed and make it difficult for management to claim they were unaware of the problems that led to the restatement.

Turning our attention to potential private enforcement mechanisms, after controlling for restatement severity and other relevant factors, we find that class action lawsuits are 5 to 10 percent more likely when firms report internal control weaknesses prior to restatements. This is true even when we remove lawsuits that are later dismissed, which is consistent with control weaknesses reported in advance of restatements lowering plaintiffs' burden of proof in showing that management was aware (or should have been aware) of the likelihood of misstatements.

We also find that top management turnover is 15 to 26 percent more likely at firms that report control weaknesses prior to their restatements. This result is consistent with firms seeking to improve the credibility of their financial reporting by replacing managers that were clearly aware of existing problems with controls over financial reporting, yet did not prevent them from manifesting in misstatements. This result holds for both CEOs and CFOs.

Likewise, auditor turnover is 6 to 9 percent more likely at firms that report control weaknesses prior to their restatements. However, this result appears to be driven primarily by auditor resignations (rather than dismissals), which is consistent with auditors opting to resign from riskier clients in the event of a restatement (e.g., Huang and Scholz 2012). Auditors perhaps view such clients as higher risk because, despite being aware of the existing control weaknesses, the auditors remained unable to successfully prevent material misstatements from appearing in these clients' financial statements.

This study makes three primary contributions to the accounting literature. First, our evidence showing that, all else equal, SEC sanctions following restatements are no more likely for firms that previously claimed to have effective internal controls (and, in some cases, are less likely) suggests that public enforcement of SOX 404 is unlikely to provide strong incentives to detect and disclose

[7] The Foreign Corrupt Practices Act of 1977 requires public firms to devise and maintain a system of internal accounting controls sufficient to provide reasonable assurance that transactions are recorded in accordance with generally accepted accounting principles (15 U.S.C. §78m(b)(2)(B)).

existing weaknesses. Second, our results showing that penalties stemming from various private mechanisms are *more* likely for firms that report their internal control weaknesses in advance of restatements suggests the existence of possible disincentives to detect and disclose existing weaknesses. Together, these results offer a potential explanation for why the majority of restatements occur at firms that previously claimed to have effective controls. Finally, we also contribute to literature that investigates various consequences of restatements (e.g., Palmrose and Scholz 2004; Palmrose, Richardson, and Scholz 2004; Desai, Hogan, and Wilkins 2006; Kravet and Shevlin 2010). Our results demonstrate that prior disclosure of control weaknesses can be an important determinant of the associated consequences and should be considered by future research.

The remainder of the paper is structured as follows. Sections II and III provide institutional background and develop our research questions and design; Section IV describes our sample; Sections V and VI present our empirical results; and Section VII concludes.

II. BACKGROUND AND RESEARCH QUESTIONS

SOX 404 requires a formal assessment of whether internal control over financial reporting is effective. Importantly, under the SEC's implementation regulations, "Management is not permitted to conclude that the registrant's internal control over financial reporting is effective if there are one or more material weaknesses" (17 C.F.R. §229.308(a)(3)). Thus, a violation of SOX 404 occurs when ineffective controls are claimed to be effective. Such misreporting can occur in several circumstances.[8] One possibility is that management is simply unaware that control weaknesses exist. That is, their control testing is insufficient to detect the weaknesses and, consequently, they do not report them.[9] A second, related possibility is that management is generally aware that a control deficiency exists, but misjudges its severity and fails to classify it as a material weakness. As shown by Bedard and Graham (2011), the classification of control deficiencies involves significant judgment, and managers often underestimate severity.[10] Finally, a third possibility is that management is aware of the weakness, but deliberately chooses not to disclose it, perhaps hoping to avoid the negative consequences that typically follow such a disclosure. As discussed in Kinney and Shepardson (2011) and Kinney, Martin, and Shepardson (2013), material weaknesses are rarely reported unless misstatements are detected during the financial audit.

For our purposes, we note that each of the above circumstances is the result of managerial and/ or auditor decision-making, which ultimately leads to inaccurate SOX 404 reporting, regardless of the level of intent. From a public policy perspective, it is not clear whether more aggressive enforcement against intentional (versus unintentional) SOX 404 misreporting would be optimal, because managers and auditors influence the level of effort expended on detecting and assessing potential weaknesses. That is, if ignorance is a valid defense, then managers and auditors have decreased incentives to detect control deficiencies or make good-faith judgments about their severity. Thus, while not all SOX 404 misreporting is intentional, intent is ultimately unobservable and, therefore, it is important to understand the consequences of misreporting more generally.[11]

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8 For ease of exposition, we frame this discussion around management, but because the auditor also issues a control opinion, it applies to auditors as well.

9 See Kinney et al. (2013) for a discussion of the challenges inherent in control testing, particularly as they relate to the difference in testing the effectiveness of a *process* (internal control) as opposed to the accuracy of a numerical *statement* (financial statement item).

10 Material weaknesses are the most severe category of control deficiency, based on both the likelihood of an associated misstatement occurring and the materiality of such a misstatement, and they are the only category required to be disclosed in SOX 404 reports.

11 While our primary focus is on the penalties related to misreporting, independent of the reasons for misreporting, in Section VI, we conduct additional analyses on a subsample where the misreporting is more likely to be intentional.

Accounting and Auditing Enforcement Releases (AAERs)

We begin our investigation of potential penalties for SOX 404 reporting failures by considering public enforcement. Section 3 of SOX grants the SEC authority to issue related regulations and enforce its various provisions. Accordingly, we focus on AAERs, which are a designation the SEC assigns to enforcement actions for accounting-related infractions. Section 3 of SOX also establishes that violations of the Act are violations of the Securities Exchange Act of 1934. As such, internal control certifications under SOX 404 are subject to the same general regulations as financial statements and other disclosures issued by registrants. In particular, Section 10(b) of the Exchange Act (15 U.S.C. §78j) and related SEC Rule 10b-5 (17 C.F.R. §240.10b-5) prohibit registrants from making untrue or misleading statements about material facts. Certifying that controls are effective, when they are later revealed to be ineffective, could, therefore, be construed as a 10b-5 violation. Hence, among firms with restatements, failures to report the existence of control weaknesses until after the restatement could be viewed as additional violations of the Exchange Act and symptomatic of deeper reporting problems. Thus, we might expect SEC enforcement actions to be more likely for firms with restatements that previously claimed to have effective controls.

However, there are also reasons to expect that the previous revelation of internal control weaknesses might instead increase the likelihood of an SEC enforcement action among firms with restatements. In particular, the SEC often prosecutes accounting violations under the FCPA, which requires both that firms keep accurate books and records, and that they devise and maintain a system of internal controls sufficient to provide reasonable assurances that financial statements are prepared in accordance with generally accepted accounting principles. These are commonly referred to as the books and records provision and the internal controls provision, respectively, of the FCPA (15 U.S.C. §78m(b)(2)(A) and (B)). Most AAERs allege violations of one or both of these provisions.[12]

The SEC regularly reviews financial statements and other public disclosures and pursues investigations in those cases where the probability of success is highest (e.g., Feroz, Park, and Pastena 1991; Files 2012). Because the disclosure of material weakness in internal controls effectively acknowledges that the firm is in violation of the control provisions of the FCPA, and because control weaknesses often signal potential accounting problems, we might expect that they increase the likelihood of an SEC investigation and resulting AAER. Moreover, previously reported control weaknesses can undermine "plausible deniability" by making it more difficult for management to claim later that they were unaware of the underlying problems that resulted in misstatements. Thus, whether the timely reporting of control weaknesses under SOX 404 decreases or increases the likelihood of an AAER is an empirical question.

Class Action Lawsuits

The remainder of our investigation focuses on various private mechanisms that could potentially serve to enforce SOX 404. The first such mechanism we consider is class action lawsuits filed by investors against restating firms. These lawsuits generally center on allegations of 10b-5 violations. In these claims, the plaintiffs plead that the defendant made a material misstatement or omission and that the plaintiffs' reliance on that misstatement or omission led to injury in connection with the purchase or sale of securities. These conditions are typically evidenced by the restatement of prior financial statements and related stock price drop. However, in order to survive the inevitable motions to dismiss, plaintiffs must also show that the defendants acted with intent or reckless

[12] Despite its name, the Foreign Corrupt Practices Act applies to all U.S. issuers of securities. Thus, firms need not have foreign operations for these provisions to apply. Of the 43 firms in our sample that received AAERs, 41 were cited for violating the internal control provisions of the FCPA.

disregard (scienter). Establishing scienter is generally the most significant hurdle facing plaintiffs in accounting-related class actions (e.g., Pritchard and Sale 2005; Johnson, Nelson, and Pritchard 2006; Donelson, McInnis, and Mergenthaler 2012). If the plaintiffs can establish scienter and survive the motion to dismiss, then these cases are almost always settled rather than going to trial.

A priori, whether timely control weakness reporting decreases or increases the likelihood of a class action lawsuit following a restatement is unclear. On one hand, failure to report an existing material weakness represents an additional misstatement that can potentially be used by plaintiffs as evidence of misleading reporting (in addition to the misstated financial statements). Because violations of SOX are considered violations of the Exchange Act, misreporting on the effectiveness of internal controls can subject firms to 10b-5 claims, which some legal scholars suggest could significantly increase the likelihood of litigation (e.g., Butler and Ribstein [2006] characterize the potential liability for failing to report control weaknesses as a "litigation time bomb").[13] Hence, timely reporting of control weaknesses may be viewed as a signal of management acting in good faith and help insulate the firm from claims that management knowingly misled investors. Likewise, reporting an existing control weakness could indirectly mitigate the risk of a related lawsuit by decreasing the amount of time the stock trades at inflated prices, thereby lessening the severity of a single large price drop at the restatement announcement. These arguments suggest that firms are less likely to face class action lawsuits following a restatement if they have previously reported their internal control weaknesses.

Alternatively, there are at least two reasons to expect that reporting the existence of control weaknesses could instead expose firms to a greater likelihood of litigation in the event of a restatement. First, the previous disclosure of material weaknesses in controls could increase the likelihood of scienter being established by lowering plaintiffs' burden of proof in showing that management was aware (or should have been aware) of the likelihood of misstatements. By having already acknowledged material weaknesses in internal controls, it becomes more difficult for management to reasonably claim that they were unaware of associated misstatements in the financial reports.[14] Therefore, if previous disclosure of control weaknesses increases plaintiffs' likelihood of succeeding in a class action, then it is likely to encourage lawsuit filings.

Second, reported control weaknesses can bring the firm to the attention of class action litigators as a potential target. For example, Files, Swanson, and Tse (2009) argue that class action law firms typically have staff specifically assigned to searching for news stories and other disclosures to identify firms with lawsuit potential and, as a result, prominent disclosure of restatement information increases the likelihood of restating firms becoming targets. Consistent with this argument, they show that, all else equal, firms that disclose restatements prominently in press

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[13] As an example, Weatherford International was sued in a class action following a 2011 restatement. Prior to the restatement, Weatherford had claimed that its internal controls were effective, but after the restatement, Weatherford acknowledged that material weaknesses did indeed exist. Investors sued, alleging misrepresentations in both the accounting and the internal control reports. The judge dismissed the accounting allegations, citing insufficient evidence of scienter, but upheld the internal control allegations. The internal control violations alone carried the suit. It was settled for $52.5 million.

[14] For example, following a restatement in 2007, Shuffle Master, Inc., which had reported material weaknesses in its internal controls prior to the restatement, was sued in a class action. As part of the scienter allegations, the plaintiffs argued, "The Defendants knew or with deliberate and extreme recklessness disregarded the fact that the Company had not corrected the previously disclosed deficiencies in its internal controls and that the Company's internal controls therefore continued to suffer from systemic weaknesses that rendered the Company's financial accounting and reporting less reliable; thus, the Defendants knew of or deliberately disregarded the risk that accounting would be improper in order to 'meet the numbers.'" The court agreed that management's knowledge of the control weaknesses (as evidenced by their SOX 404 reports), and failure to prevent those weaknesses from manifesting in misstatements, created a strong inference of scienter and denied Shuffle Master's motion to dismiss (see http://securities.stanford.edu/filings-case.html?id=103771).

releases are more likely to be sued than those with stealth restatements. In our setting, given the link between control weaknesses and potential accounting problems, adverse SOX 404 opinions could also serve to draw the attention of law firms, thereby increasing the likelihood of litigation in the event of a subsequent restatement.

Given the opposing arguments outlined above, we do not predict the direction of the association between timely control weakness reporting and the likelihood of class action lawsuits against firms with restatements.

Management Turnover

We also consider employment-related consequences for managers. In particular, we examine whether the likelihood that a restatement leads to management turnover is affected by whether internal control weaknesses are reported in advance of the restatement. If the failure to report existing weaknesses is perceived as a sign of managerial incompetence or intent to deceive, then we might expect management turnover to be more likely for firms that claimed to have effective controls prior to their restatement. Alternatively, Li, Sun, and Ettredge (2010) show that, in general, management turnover is more likely for firms with internal control weaknesses as boards seek to improve the perceived credibility of their financial reporting. While control problems exist for all of our sample firms, at those firms whose weaknesses were reported prior to their restatement, management was clearly aware of the control problems, yet was unable to prevent them from manifesting in misstatements. As a result, we might expect management turnover to be more likely at firms with reported control weaknesses. Given the conflicting arguments, which effect dominates, if any, becomes an empirical question.

Auditor Turnover

Finally, we also consider consequences for auditors. In particular, we examine whether the likelihood of auditor turnover following a restatement is affected by the previous disclosure of control weaknesses. Because opining that a firm has effective internal controls when, instead, material weaknesses exist suggests a failure on the part of the audit firm, we might expect auditor turnover to be higher at firms that previously received clean SOX 404 opinions. Alternatively, previous research suggests that reported control weaknesses increase the likelihood of auditor turnover as firms dismiss auditors to enhance the credibility of their financial reporting, and as auditors resign from their riskier clients (Ettredge, Heintz, Li, and Scholz 2011; Huang and Scholz 2012). We might expect both of these effects (dismissals and resignations) to be stronger in cases where auditors were aware of control weaknesses (as evidenced by their prior adverse SOX 404 opinions), yet were unable to prevent misstatements from occurring in the financial statements. Therefore, we make no prediction on the direction of the association between auditor turnover and the reporting of existing control weaknesses.

III. RESEARCH DESIGN

Our research design centers on estimating variants of the following basic model:

$$PENALTY = f(REPORT\ ICW,\ RESTATEMENT\ SEVERITY,\ FIRM\ CHARACTERISTICS\ AND\ OTHER\ CONTROLS), \tag{1}$$

where *PENALTY* represents one of the potential enforcement mechanisms we examine (i.e., AAER, litigation, management turnover, or auditor turnover). The details for each specific response variable are explained more fully below (all variable definitions and data sources are also provided in Appendix A). Our primary independent variable of interest is *REPORT ICW*, which is an

indicator for whether firms reported control weaknesses in advance of their restatement (coded 1 for those that did, and 0 for those that did not). In our primary analyses, we use probit regression to estimate Equation (1).

The first set of controls in Equation (1) relate to restatement severity. All else equal, we expect that more severe restatements increase the likelihood of the various penalties. Our controls for restatement severity include the change in reported income (*REST MAGNITUDE*), the number of distinct accounts being restated (*REST COUNT*), the number of years being restated (*REST YEARS*), an indicator for whether reported revenue was restated (*REST REVENUE*), and the abnormal stock return at the restatement announcement (*CAR*). Following Hennes, Leone, and Miller (2008), we also control for whether restatements are attributable to unintentional errors or intentional irregularities. *IRREGULARITY* is coded 1 for restatements related to fraud or where SEC or board-instigated independent investigations occur, and 0 otherwise.

We also include controls for relevant firm characteristics and other factors in Equation (1). These controls are specific to each individual penalty variable, as described below.[15]

AAERs

We collect AAER information from the SEC's website (see, http://www.sec.gov) and create the variable *AAER*, which we code as 1 for firms that were the subject of an enforcement action related to their restatement, and 0 otherwise. In addition to the restatement severity controls, we also include control variables related to firm size, visibility, and the severity of related investor losses in the AAER model because SEC enforcement is likely to be affected by these factors (e.g., Karpoff, Lee, and Martin 2007).[16] Since larger firms are more likely to be targets of enforcement, we include *SIZE*, measured as the natural log of the market value of common equity. We control for investor losses leading up to the restatement with *PREVIOUS RETURN*, which is calculated as the buy-and-hold abnormal return using the CRSP equally weighted market portfolio return measured over the window (−252, −2) relative to the restatement announcement. We also control for how actively a firm's shares are traded using *SHARE TURNOVER*, measured as $[1 - \Pi_t(1 - \textit{shares traded}_t/\textit{total shares}_t)]$ over the one-year period ending on the second day prior to the restatement announcement date (Field, Lowry, and Shu 2005; Files et al. 2009), and for the growth in sales over the last year of the misstatement period (*SALES GROWTH*). We expect both *SHARE TURNOVER* and *SALES GROWTH* to be positively associated with the likelihood of an AAER.

Class Action Lawsuits

We collect lawsuit data from the Stanford Securities Class Action Clearing House and create the indicator variable *LITIGATION*, which we code as 1 if a class action lawsuit related to the restatement is filed against the firm, and 0 otherwise. Because many lawsuits are dismissed, we also create *LIT EXCL DISMISS*, which is coded as 1 only for those lawsuits that are not dismissed, and 0 otherwise.

Our litigation models include the controls for restatement severity, as well as several factors that prior research suggests are related to the likelihood of litigation more generally (e.g., Kim and Skinner 2012; Rogers and Stocken 2005). As in the AAER model, we include *PREVIOUS RETURN, SHARE*

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[15] The specific controls included in each model are primarily motivated by previous literature and are based on the nature of the specific penalty measure in question. Thus, they vary somewhat depending on which dependent variable is being examined. In untabulated tests, we include the full set of all controls in each model. The controls that are omitted from our tabulated results are generally insignificant and have little impact on our main results.

[16] We omit *IRREGULARITY* from the AAER model because it is mechanically related to the dependent variable in this particular version of Equation (1). Recall that part of the Hennes et al. (2008) irregularity classification scheme is based on whether there is an SEC investigation, which is a precursor to an AAER.

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TURNOVER, SIZE, and *SALES GROWTH.* We expect litigation to be negatively associated with *PREVIOUS RETURN* and positively associated with the others. We also control for the standard deviation and skewness of stock returns preceding the restatement, expecting that the likelihood of litigation is positively associated with standard deviation and negatively associated with skewness (Kim and Skinner 2012). Similar to *PREVIOUS RETURN,* we measure *RETURN STD DEV* and *RETURN SKEWNESS* over the window (−252, −2) relative to the restatement announcement. We include *BIG4,* an indicator for whether the firm's auditor is one of the four largest audit firms (Deloitte, Ernst & Young, KPMG, and PricewaterhouseCoopers), to control for possible deep pockets effects related to the size of the auditor. Finally, we include an indicator variable, *LIT INDUSTRY,* for firms in industries identified by Francis, Philbrick, and Schipper (1994) as having higher litigation risk, and expect it to be positively associated with the likelihood of litigation.

Management Turnover

We identify top management turnover using Audit Analytics and code *MGT TURN* as 1 if there is a change in either CEO or CFO during the one-year period following the restatement announcement, and as 0 otherwise. We also consider CEO and CFO turnover separately. Along with the restatement severity controls, our management turnover models include controls for firm size (*SIZE*), stock returns preceding the restatement announcement (*PREVIOUS RETURN*), and return on assets for the year prior to the restatement as a proxy for recent operating performance (*ROA*). We expect management turnover to be more likely at firms with poor recent stock returns and operating performance, but do not make a prediction for firm size.

When we estimate Equation (1) with *MGT TURN* as the dependent variable, we remove observations with management changes in the year prior to the restatement. This eliminates cases where a restatement was potentially prompted by a new manager who disagrees with the firm's previous accounting practices, and also ensures that the managers in question had signed off on at least one set of previous, misstated financial statements (and associated SOX 404 reports).

Auditor Turnover

Similar to management turnover, we use Audit Analytics data to code *AUD TURN* as 1 if there is an auditor change during the one-year period following the restatement announcement, and 0 otherwise. We also consider auditor dismissals and resignations separately. In addition to the controls for restatement severity, we include *PREVIOUS RETURN* in the auditor turnover models, and expect turnover to be more likely at firms with lower stock returns. We also include *SALES GROWTH*, and expect that higher-growth firms are more likely to change auditors (Johnson and Lys 1990). Finally, because previous research suggests that the costs of switching auditors are greater for larger firms and those with Big 4 auditors (e.g., Hennes, Leone, and Miller 2014), we include *SIZE* and *BIG4* as controls.

Following Hennes et al. (2014), and similar to our approach with management turnover, we remove observations with auditor changes in the year prior to the restatement when estimating Equation (1) with *AUD TURN* as the dependent variable. This eliminates cases where a restatement was prompted by an auditor switch (rather than *vice versa*) and ensures that the auditors in question have signed off on at least one set of previous, misstated financial statements and the associated SOX 404 reports.

Controlling for Self-Selection

The treatment in our research design (i.e., *REPORT ICW*) is not randomly assigned and, therefore, we also acknowledge the possibility that the reporting of existing control weaknesses could be endogenous. If there are factors that affect the reporting of existing weaknesses that are

also correlated with the various penalties we examine, then the coefficient estimates from our probit models could be biased. To address this possibility, we use propensity score-matching to create an alternative sample where firms are matched on factors that affect the likelihood of reporting existing control weaknesses (Rosenbaum and Rubin 1983). We estimate propensities using the model of Rice and Weber (2012), who model the probability of reporting existing weaknesses as a function of financial distress, external capital needs, firm size, recent manager and auditor turnover, audit firm size, audit fees, nonaudit fees paid to the audit firm, and previously reported control deficiencies and restatements. See Rice and Weber (2012, 829, Table 4) for more details. We limit matches to be within a caliper of 0.03 and perform the matching with replacement (Morgan and Harding 2006; Roberts and Whited 2011).

By first modeling firms' propensities to report their existing weaknesses and then matching firms based on those propensities, we control for the known determinants of *REPORT ICW*. This provides reasonable assurance that our results are not attributable to observable differences between firms that report existing weaknesses and those that do not.[17]

IV. SAMPLE

Sample Selection

Our sample selection centers on identifying firms that are subject to SOX 404 and that also have restatements. We then match the restatements with the SOX 404 reports issued by these firms during their misstatement periods (i.e., the periods for which misstatements appeared in their financial statements that are later corrected in their restatements). This enables us to determine whether control weaknesses were reported prior to the restatements.

Restatements and SOX 404 reports are both drawn from Audit Analytics. We begin by extracting all restatements for U.S.-incorporated firms announced by the end of 2010 that include annual reporting periods ending after the effective date of SOX 404 (November 14, 2004). For firms with multiple restatements announced during this period, we keep only the first instance. We then match the restatements with the SOX 404 reports issued during each firm's misstatement period.[18] This process results in 834 restatement observations with matching SOX 404 reports. From this initial sample, we eliminate 31 observations for which the restatement announcement date precedes the end of the misstatement period in Audit Analytics.[19] We also eliminate 144 observations that are missing data necessary to construct the variables in our models, resulting in a usable sample of 659 observations. We refer to this as our full sample. The full sample includes 134 firms that reported the existence of material weaknesses prior to their restatements and 525 that did not.

While restatements are generally indicative of deficiencies in the control environment, in some less-common situations, the occurrence of a restatement may not strictly imply the existence of a material weakness.[20] Thus, while most of the analyses we present are based on the full sample, as a

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[17] In Section VI, we also describe a bivariate probit approach that considers the possibility of endogeneity due to other, unobservable factors.

[18] We require the SOX 404 reports to be audited, a regulation that only applies to accelerated filers. Thus, all our sample firms are accelerated filers (public float in excess of $75 million).

[19] These appear to be largely situations where firms first announce an internal investigation of some accounting issue that has the potential to ultimately result in a restatement. Audit Analytics marks this initial announcement as the restatement disclosure date. However, in some cases, the investigations are protracted and details of the restatement become available only with some considerable delay. Thus, we eliminate these observations.

[20] For example, the PCAOB (2004, para. E99) allows that a restatement may not imply a material weakness if it "reflected the SEC's subsequent view of an accounting matter, when the auditor concluded that management had reasonable support for its original position." Likewise, some restatements correct relatively minor clerical errors, which may not rise to the level of a material weakness in internal controls (Rice and Weber 2012).

sensitivity check, we also construct a subsample constrained to only those observations for which we can use the firms' own disclosures to unambiguously link their restatements to underlying material weaknesses. We refer to this as our constrained sample.

For the 525 firms from the full sample that claimed to have effective internal controls during their misstatement periods (i.e., did not report any material weaknesses prior to their restatement), we examine the control-related disclosures issued in the wake of their restatements. Of the firms in this group, 260 explicitly acknowledge the existence of material weaknesses in the immediate aftermath of their restatements. Another 54 firms report that material weaknesses existed during their misstatement period, but have since been remediated. Despite initially reporting that their internal controls were effective, these 314 (260 + 54) firms explicitly acknowledge in hindsight that material weaknesses did indeed exist and we retain them for our constrained sample. We eliminate the other 211 firms that either do not mention (176 firms) or explicitly deny (35 firms) the existence of control weaknesses after their restatement.

We also examine the 134 firms from the full sample that reported control weaknesses in advance of their restatements. We first attempt to link the particular type of misstatements being corrected with the type of control weaknesses that were originally reported (e.g., a revenue restatement and a reported weakness in controls over revenue). For those where a link is not obvious, we read the control-related disclosures issued in the aftermath of the restatement and eliminate 14 cases where management either denies that the restatement is linked to a control weakness or does not mention such a link. Hence, our constrained subsample consists of 434 observations, including 120 firms that acknowledged control weaknesses in advance of their restatements and 314 that acknowledged them only in hindsight.

Descriptive Statistics

In Table 1, we provide descriptive statistics for our regression variables. Our sample firms are relatively large, with a median value for *SIZE* of 6.470 (which translates to approximately $645 million in market capitalization), and a large majority (85 percent) are audited by one of the four largest audit firms. Approximately 10 percent of sample firms face litigation as a result of their restatement, although the mean value of *LIT EXCL DISMISS* (0.061) indicates that a large proportion of these cases are ultimately dismissed. About 7 percent of the sample is sanctioned by the SEC with an AAER, while the management and auditor turnover percentages are approximately 28 and 10 percent, respectively.

Table 1 also includes the mean values of our variables conditional on *REPORT ICW*. Turnover rates are higher for both managers and auditors of firms that previously reported their control weaknesses (i.e., *REPORT ICW* = 1). The differences in litigation and AAER rates across groups are not significant. Taken together, there is no evidence from these univariate comparisons to suggest a higher likelihood of penalties for firms that claimed to have effective internal controls prior to their restatement. To the contrary, management and auditor turnover rates are instead higher for firms that report their control weaknesses in a timely manner. These simple comparisons, however, do not control for other potentially relevant factors and, accordingly, we base our inferences on the regression results in the next section.

Restatement severity appears to be reasonably similar across *REPORT ICW* groups, with insignificant differences for most of our proxies (*IRREGULARITY*, which is larger for *REPORT ICW* = 1 firms, is the only exception). Regarding firm characteristics, the *REPORT ICW* = 0 firms in our sample tend to be larger and more likely to have a Big 4 auditor. Other characteristics are similar across groups.

V. RESULTS

Our primary regression results are presented in Tables 2–5, with each table focused on a different type of penalty serving as the dependent variable in Equation (1). Along with coefficient

TABLE 1
Descriptive Statistics

	Full Sample						REPORT ICW = 1		REPORT ICW = 0	
Variable	Mean	Std. Dev.	25th Pctl.	Med.	75th Pctl.	n	Mean	n	Mean	n
Penalties										
AAER	0.065	0.247	0.000	0.000	0.000	659	0.060	134	0.067	525
LITIGATION	0.099	0.298	0.000	0.000	0.000	659	0.127	134	0.091	525
LIT EXCL DISMISS	0.061	0.239	0.000	0.000	0.000	659	0.082	134	0.055	525
MGT TURN	0.277	0.448	0.000	0.000	1.000	487	0.386	83	0.255**	404
AUD TURN	0.097	0.296	0.000	0.000	0.000	601	0.162	111	0.082***	490
Restatement Severity Controls										
REST MAGNITUDE	–0.015	0.044	–0.012	–0.002	0.000	659	–0.019	134	–0.014	525
IRREGULARITY	0.686	0.465	0.000	1.000	1.000	659	0.776	134	0.663**	525
REST REVENUE	0.153	0.361	0.000	0.000	0.000	659	0.157	134	0.152	525
REST COUNT	2.331	1.551	1.000	2.000	3.000	659	2.246	134	2.352	525
REST YEARS	3.242	2.500	1.496	2.745	4.000	659	3.474	134	3.182	525
CAR	–0.022	0.075	–0.042	–0.010	0.014	659	–0.025	134	–0.021	525
Firm Characteristics and Other Controls										
LIT INDUSTRY	0.329	0.470	0.000	0.000	1.000	659	0.381	134	0.316	525
PREVIOUS RETURN	–0.100	0.447	–0.377	–0.139	0.111	659	–0.088	134	–0.103	525
RETURN STD DEV	0.031	0.018	0.019	0.026	0.035	659	0.033	134	0.031	525
RETURN SKEWNESS	0.267	1.063	–0.174	0.273	0.715	659	0.207	134	0.283	525
SHARE TURNOVER	0.804	0.201	0.689	0.885	0.963	659	0.800	134	0.805	525
SIZE	6.573	1.422	5.548	6.470	7.384	659	6.162	134	6.678***	525
BIG4	0.850	0.358	1.000	1.000	1.000	659	0.716	134	0.884***	525
SALES GROWTH	0.078	0.232	–0.014	0.035	0.154	659	0.087	134	0.076	525
ROA	–0.005	0.169	–0.016	0.020	0.066	659	–0.010	134	–0.003	525

***, **, * Represent statistically significant differences between the *REPORT ICW* = 1 and *REPORT ICW* = 0 groups at the 0.01, 0.05, and 0.10 levels, respectively.
The samples for *MGT TURN* and *AUD TURN* are reduced by the elimination of observations with management and auditor turnover, respectively, during the year preceding the restatement.
Variables are as defined in Appendix A.

estimates and standard errors, we also include estimated marginal effects for each variable in these tables. The reported marginal effects are averages across all observations in the sample (Bartus 2005; Greene 2003). For binary independent variables, they represent the average change in the probability of the penalty occurring for a change in the independent variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values, to ease interpretation and comparison across variables of different scales.

AAERs

In Table 2, we provide the results of our AAER regressions. The first two sets of results represent the full sample and the propensity score-matched sample, respectively. In both cases, the estimated coefficients on *REPORT ICW* are positive, but only the coefficient for the propensity score-matched sample is statistically significant. The associated marginal effect indicates that, all else equal, firms in the propensity score-matched sample are about 6 percent more likely to receive an AAER following a restatement if they have also previously disclosed control weaknesses. The results for the control variables are largely in line with expectations. Firms with more severe restatements and more negative stock returns and those that are larger, more heavily traded, and higher-growth are generally more likely to receive an AAER.

Because the SEC is unlikely to pursue sanctions for relatively minor violations, regardless of *REPORT ICW*, we also estimate the AAER model on a subset of firms for which the potential damages to investors are particularly large. Following Files (2012), we calculate *DAMAGES* for each firm as the difference between market capitalization at its highest point during the misstatement period and its value on the day after the restatement announcement. This measure incorporates both the size of the firm and its stock market losses. The third set of results in Table 2 represents the AAER model estimated on the subsample of firms with above-median *DAMAGES*. The estimated coefficient on *REPORT ICW* is again insignificant. Therefore, even within this subsample, we find no evidence that the failure to report control weaknesses prior to a restatement increases the likelihood of SEC sanctions.

Finally, as a sensitivity check, we estimate the AAER model using the constrained sample, which includes only those firms that explicitly acknowledge that their restatements are attributable to material control weaknesses. The results are presented in the final columns of Table 2. Consistent with the full sample, the estimated coefficient on *REPORT ICW* is positive, but not statistically significant.

Overall, there is no evidence of vigorous public enforcement of SOX 404, as restating firms are no more likely to face SEC sanctions if they also failed to report their existing control weaknesses until after the restatement. If anything, the evidence is weakly suggestive of the opposite: that reported control weaknesses aid the SEC in identifying cases where potential enforcement actions are likely to succeed, and provide a tacit acknowledgement of FCPA violations that makes it more difficult for management to claim they were unaware of the problems that later led to a restatement.

Class Action Lawsuits

We present our analyses of class action lawsuits in Table 3. As a starting point, we begin with *LITIGATION* as the dependent variable for the first model, but to ensure that our results are not simply driven by lawsuits that are later dismissed, we then move to *LIT EXCL DISMISS* as the dependent variable for the remainder of the models.[21]

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[21] We omit *IRREGULARITY* from the models where *LIT EXCL DISMISS* is the dependent variable because every observation in our sample with *LIT EXCL DISMISS* = 1 also has *IRREGULARITY* = 1. In Section VI, we perform sensitivity analysis using subsamples of only firms with irregularities.

TABLE 2

AAER Regressions

		Dependent Variable: *AAER*							
		Full Sample		**Propensity Score-Matched Sample**		**Subsample with Above-Median *DAMAGES***		**Constrained Sample**	
Parameter	**Pred. Sign**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**
Intercept	+/–	–4.413***		–20.976***		–4.221***		–5.335***	
		(0.737)		(7.056)		(1.120)		(1.029)	
REPORT ICW	+/–	0.012	0.001	1.461**	0.058	–0.233	–0.029	0.036	0.004
		(0.235)		(0.694)		(0.310)		(0.253)	
REST MAGNITUDE	–	–5.363***	–0.006	–13.895**	–0.007	–5.529***	–0.014	–4.999***	–0.010
		(1.662)		(6.954)		(2.021)		(1.683)	
REST REVENUE	+	0.765***	0.093	3.012***	0.198	1.002***	0.176	0.769***	0.103
		(0.218)		(1.171)		(0.281)		(0.247)	
REST COUNT	+	0.047	0.008	0.303*	0.023	0.051	0.013	0.020	0.004
		(0.056)		(0.190)		(0.070)		(0.065)	
REST YEARS	+	0.131***	0.025	0.300***	0.025	0.155***	0.058	0.110***	0.032
		(0.032)		(0.116)		(0.043)		(0.040)	
CAR	–	–3.268***	–0.015	–3.303	–0.007	–3.267***	–0.028	–4.161***	–0.027
		(0.961)		(2.782)		(1.141)		(1.075)	
PREVIOUS RETURN	–	–0.486**	–0.022	–0.362	–0.010	–0.391	–0.024	–0.669***	–0.035
		(0.243)		(0.600)		(0.322)		(0.285)	
SHARE TURNOVER	+	0.303	0.007	10.244**	0.072	0.290	0.006	1.074	0.031
		(0.617)		(5.146)		(0.900)		(0.869)	
SIZE	+	0.190***	0.030	0.688**	0.027	0.148*	0.035	0.241***	0.043
		(0.073)		(0.346)		(0.090)		(0.097)	
SALES GROWTH	+	1.028***	0.015	1.146	0.009	1.253***	0.026	0.836**	0.015
		(0.378)		(1.679)		(0.494)		(0.461)	
Observations		659		214		321		434	
Likelihood ratio Chi-squared		88.52		43.14		56.43		72.56	
Pseudo R^2		0.278		0.578		0.265		0.292	

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American Accounting Association

TABLE 2 (continued)

***, **, * Denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs, and two-tailed otherwise.
This table presents the results of AAER regressions using probit estimation. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of an AAER occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values.
Variables are as defined in Appendix A.

TABLE 3

Litigation Regressions

		Dependent Variable: *LITIGATION*		Dependent Variable: *LIT EXCL DISMISS*							
		Full Sample		Full Sample		Propensity Score-Matched Sample		Subsample with Above-Median *DAMAGES*		Constrained Sample	
Parameter	Pred. Sign	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect
Intercept	+/–	–7.903***		–6.209***		–19.852***		–10.803***		–8.448***	
		(1.024)		(1.026)		(7.145)		(2.306)		(1.490)	
REPORT ICW	+/–	0.415*	0.050	0.491**	0.048	1.535*	0.073	0.829**	0.102	0.429	0.048
		(0.216)		(0.239)		(0.786)		(0.340)		(0.262)	
REST MAGNITUDE	–	–1.416	–0.002	–2.271	–0.002	–4.476	–0.004	–2.612	–0.005	–1.964	–0.004
		(1.686)		(1.887)		(4.938)		(2.547)		(1.963)	
IRREGULARITY	+	1.434***	0.106								
		(0.399)									
REST REVENUE	+	0.638***	0.085	0.428**	0.042	0.269	0.016	0.447	0.051	0.511**	0.060
		(0.215)		(0.244)		(0.713)		(0.351)		(0.277)	
REST COUNT	+	0.067*	0.014	0.115**	0.018	0.515**	0.046	0.218***	0.040	0.105*	0.020
		(0.051)		(0.057)		(0.224)		(0.084)		(0.065)	
REST YEARS	+	0.076**	0.019	0.116***	0.020	0.087	0.013	0.161***	0.047	0.129***	0.034
		(0.033)		(0.036)		(0.094)		(0.052)		(0.045)	
CAR	–	–5.398***	–0.032	–5.314***	–0.022	–9.461***	–0.022	–5.959***	–0.036	–6.032***	–0.036
		(1.033)		(1.048)		(3.108)		(1.381)		(1.224)	
LIT INDUSTRY	+	–0.189	–0.020	–0.295	–0.024	0.696	0.041	–0.456	–0.044	–0.345	–0.034
		(0.194)		(0.220)		(0.616)		(0.297)		(0.250)	
PREVIOUS RETURN	–	–0.604***	–0.033	–0.513**	–0.021	–0.975*	–0.037	–1.175***	–0.055	–0.460*	–0.023
		(0.228)		(0.253)		(0.692)		(0.408)		(0.289)	
RETURN STD DEV	+	11.879**	0.019	8.505	0.011	34.782**	0.038	10.497	0.019	17.277**	0.025
		(6.661)		(6.780)		(17.243)		(9.137)		(8.169)	

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TABLE 3 (continued)

		Dependent Variable: *LITIGATION*		Dependent Variable: *LIT EXCL DISMISS*							
		Full Sample		Full Sample		Propensity Score-Matched Sample		Subsample with Above-Median *DAMAGES*		Constrained Sample	
Parameter	**Pred. Sign**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**	**Est. Coeff. (Std. Error)**	**Marg. Effect**
RETURN SKEWNESS	–	0.196	0.019	0.195	0.014	0.542	0.025	0.312	0.031	0.183	0.018
		(0.083)		(0.093)		(0.333)		(0.122)		(0.101)	
SHARE TURNOVER	+	1.478***	0.045	1.035*	0.024	7.899**	0.086	3.103**	0.053	2.129**	0.054
		(0.624)		(0.713)		(4.323)		(1.524)		(1.035)	
SIZE	+	0.305***	0.057	0.234***	0.033	0.740**	0.041	0.410***	0.070	0.371***	0.059
		(0.080)		(0.088)		(0.362)		(0.137)		(0.115)	
BIG4	+	0.611*	0.054	0.509	0.034	0.696	0.034	0.946	0.065	0.598	0.048
		(0.399)		(0.444)		(0.890)		(0.795)		(0.519)	
SALES GROWTH	+	0.292	0.005	0.792**	0.011	0.044	0.001	0.969**	0.015	0.701*	0.012
		(0.396)		(0.413)		(1.354)		(0.552)		(0.499)	
Observations		659		659		214		321		434	
Likelihood ratio Chi-squared		160.10		95.80		47.78		71.27		90.53	
Pseudo R^2		0.377		0.318		0.517		0.375		0.358	

***, **, * Denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs, and two-tailed otherwise.
This table presents the results of litigation regressions using probit estimation. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of litigation occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values.
Variables are as defined in Appendix A.

The first set of results in Table 3 are for the full sample with *LITIGATION* as the dependent variable, and the next two sets are for the full and propensity score-matched samples, respectively, with *LIT EXCL DISMISS* as the dependent variable. The estimated coefficients on *REPORT ICW* are positive and significant in each of these first three models, indicating that firms reporting control weaknesses prior to their restatements are more likely to face class action lawsuits. The estimated marginal effects suggest that *REPORT ICW* = 1 firms are roughly 5 to 7 percent more likely to face litigation. These effects are also economically significant, considering that the sample averages for *LITIGATION* and *LIT EXCL DISMISS* are about 10 and 6 percent, respectively (see Table 1). Results for the control variables are generally consistent with expectations. Lawsuits are more likely following more severe restatements, for larger and more heavily traded firms, and when stock returns are more negative.

As with our AAER analysis, we also separately examine the subsample of firms with *DAMAGES* above the sample median, as these firms are most likely to be potential targets of class actions. The estimated coefficient in Table 3 on *REPORT ICW* is again positive and significant for this group, with an associated marginal effect of about 10 percent.

The final set of results in Table 3 is for the constrained sample. The estimated coefficient on *REPORT ICW* is again positive, but is not statistically significant (two-tailed p-value = 0.1009). However, we note that the marginal effect is the same magnitude as for the full sample (4.8 percent), and if we limit the constrained sample to only those with *DAMAGES* over the median, then the coefficient on *REPORT ICW* is again positive and significant (untabulated).

As a whole, the Table 3 results are consistent with the notion that reported control weaknesses serve to raise the awareness of class action litigators as to the potential for a given firm to be a viable target, regardless of whether the nature of the reported weakness is directly tied to the restatement. Moreover, the fact that the results hold for suits that survive motions to dismiss (*LIT EXCL DISMISS*) is consistent with previously acknowledged control weaknesses lowering plaintiffs' burden in establishing scienter by making it more difficult for management to reasonably claim that they were unaware of the conditions that led to misstatements in the financial reports.[22]

Management Turnover

We present the management turnover results in Table 4. The first three sets of results all have *MGT TURN* as the dependent variable and are for the full, propensity score-matched, and constrained samples, respectively. In each case, the estimated coefficient on *REPORT ICW* is positive and significant at $p < 0.01$. The associated marginal effects indicate that management turnover is between 15 and 26 percent more likely at firms that report control weaknesses prior to their restatements. Regarding control variables, poorer performing firms and firms with more severe restatements are generally more likely to have management turnover.

The final results in Table 4 examine CEO and CFO turnover separately. In both cases, the estimated coefficient on *REPORT ICW* is positive and statistically significant. The marginal effects for CEOs and CFOs are roughly 7 and 10 percent, respectively. Thus, the general results for *MGT TURN* apply to both CEOs and CFOs, with a slightly larger effect for CFOs.

Overall, the Table 4 results are consistent with reported control weaknesses leading to increased turnover as firms seek to improve the credibility of their financial reporting by replacing

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22 In a concurrent study, Hogan, Lambert, and Schmidt (2013) also examine whether management's internal control certifications affect the likelihood of litigation. Consistent with our results, they conclude that litigation is unlikely to provide strong incentives to disclose control weaknesses in a timely manner. Our study differs from Hogan et al. (2013) in several ways, most notably in that they focus solely on class action lawsuits while we consider a range of potential consequences surrounding SOX 404 reporting.

TABLE 4

Management Turnover Regressions

Dependent Variable: *MGMT TURN*

Parameter	Pred. Sign	Full Sample Est. Coeff. (Std. Error)	Full Sample Marg. Effect	Propensity Score-Matched Sample Est. Coeff. (Std. Error)	Propensity Score-Matched Sample Marg. Effect	Constrained Sample Est. Coeff. (Std. Error)	Constrained Sample Marg. Effect	CEO Turnover Only Est. Coeff. (Std. Error)	CEO Turnover Only Marg. Effect	CFO Turnover Only Est. Coeff. (Std. Error)	CFO Turnover Only Marg. Effect
Intercept	+/–	–1.858***		–0.846		–1.834***		–1.944***		–1.755***	
		(0.358)		(0.765)		(0.498)		(0.391)		(0.346)	
REPORT ICW	+/–	0.435***	0.145	0.887***	0.261	0.505***	0.166	0.300*	0.069	0.339**	0.103
		(0.162)		(0.295)		(0.183)		(0.161)		(0.159)	
REST MAGNITUDE	–	–0.134	0.000	4.099	0.021	0.539	0.003	–0.144	0.000	0.546	0.002
		(1.623)		(3.657)		(1.546)		(1.573)		(1.602)	
IRREGULARITY	+	0.102	0.031	–0.657	–0.207	0.122	0.037	0.106	0.022	–0.025	–0.007
		(0.145)		(0.317)		(0.255)		(0.159)		(0.141)	
REST REVENUE	+	–0.102	–0.031	0.541	0.169	0.002	0.001	–0.060	–0.012	0.240*	0.072
		(0.203)		(0.433)		(0.227)		(0.202)		(0.183)	
REST COUNT	+	0.057*	0.035	0.256***	0.149	0.064*	0.040	0.031	0.013	0.067**	0.037
		(0.041)		(0.088)		(0.046)		(0.044)		(0.040)	
REST YEARS	+	0.021	0.016	–0.001	–0.001	0.052*	0.048	–0.002	–0.001	0.047**	0.033
		(0.026)		(0.055)		(0.032)		(0.029)		(0.025)	
CAR	–	–3.064***	–0.053	–3.278*	–0.070	–3.921***	–0.081	–1.816**	–0.021	–1.420*	–0.020
		(0.981)		(2.232)		(1.125)		(0.904)		(0.877)	
PREVIOUS RETURN	–	–0.483***	–0.075	–0.802**	–0.098	–0.678***	–0.108	–0.543***	–0.057	–0.388***	–0.054
		(0.151)		(0.396)		(0.205)		(0.164)		(0.153)	
SIZE	+/–	0.120**	0.067	–0.063	–0.028	0.078	0.044	0.081	0.031	0.076	0.038
		(0.049)		(0.118)		(0.070)		(0.054)		(0.047)	
ROA	–	–0.648*	–0.015	1.236	0.034	–0.441	–0.011	–0.845**	–0.014	–0.219	–0.004
		(0.445)		(1.310)		(0.536)		(0.430)		(0.408)	
Observations		487		124		306		580		542	
Likelihood ratio Chi-squared		40.39		27.72		38.18		27.86		27.62	
Pseudo R^2		0.070		0.176		0.102		0.059		0.048	

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TABLE 4 (continued)

***, **, * Denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs, and two-tailed otherwise.
This table presents the results of management turnover regressions using probit estimation. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of turnover occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values. The CEO (CFO) Turnover Only column excludes observations with CEO (CFO) turnover during the year preceding the restatement. When *MGMT TURN* is the dependent variable, observations with either CEO or CFO turnover in the year preceding the restatement are excluded.
Variables are as defined in Appendix A.

managers (both CEOs and CFOs) that were clearly aware of problems that existed in the control environment, yet did not prevent them from manifesting in misstatements.

Auditor Turnover

The auditor turnover results are provided in Table 5. For both the full sample and the propensity score-matched sample, the estimated coefficient on *REPORT ICW* is positive and significant. The corresponding marginal effects suggest that auditor turnover is 6 to 9 percent more likely at firms that reported their control weaknesses prior to their restatement. As expected, auditor size (*BIG4*) is negatively associated with auditor turnover, as is firm size for the full sample. The pervasiveness of misstatements, as measured by the number of distinct account types being restated (*REST COUNT*), increases the likelihood of auditor turnover. Auditor changes are more likely following irregularities in the full sample as well. For the propensity score-matched sample, there is also some evidence that auditor turnover is more likely at firms that have been underperforming, as measured by stock returns during the year prior to the restatement (*PREVIOUS RETURN*).[23]

The results so far provide no evidence that failing to detect and disclose existing internal control weaknesses prior to a restatement increases the likelihood of auditor turnover. To the contrary, they suggest that auditor turnover is instead more likely at firms that received adverse SOX 404 opinions prior to the restatement. This result is consistent with firms seeking to improve the credibility of their financial reporting after reporting the existence of control weaknesses (e.g., Ettredge et al. 2011) or with auditors opting to resign from riskier clients (e.g., Huang and Scholz 2012).[24] To explore these possible explanations, we separately examine auditor dismissals and resignations in the next two regressions presented in Table 5. The estimated coefficient on *REPORT ICW* is insignificant for auditor dismissals, but is positive and statistically significant for auditor resignations. The corresponding marginal effect indicates that following a restatement, auditors are about 4 percent more likely to resign from engagements with clients that have previously received an adverse SOX 404 report.

The final regression in Table 5 employs the constrained sample. We focus on auditor resignations here because the previous results suggest resignations are driving the auditor change results. The estimated coefficient on *REPORT ICW* remains positive and significant for this sample, again with a marginal effect of 4 percent.

Taken together, the results in Table 5 provide no evidence that auditors are more likely to be dismissed following the failure to detect and disclose their clients' internal control weaknesses. Instead, the results suggest that auditor changes are more likely for clients that previously received adverse SOX 404 opinions and that these changes are driven primarily by auditor resignations. Overall, the evidence appears most consistent with auditors managing risk by resigning from riskier clients, as opposed to facing penalties in the form of dismissals for failing to report control weaknesses for clients that later have restatements.[25]

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[23] In untabulated tests, we also include controls for book-to-market and bankruptcy risk (Altman Z). The additional controls are statistically insignificant and their inclusion does not affect our inferences.

[24] For example, auditors may view *REPORT ICW* = 1 clients as more risky because, despite clearly being aware of the existence of control weaknesses, the auditors remained unable to successfully prevent material misstatements from appearing in these clients' financial statements.

[25] Our tests are not designed to evaluate risk directly and, thus, our results should be viewed as suggestive rather than conclusive about this particular explanation. However, we note that untabulated tests reveal *REPORT ICW* = 1 firms are more likely than *REPORT ICW* = 0 firms to have additional restatements in subsequent years, which is consistent with heightened risk for these clients (the three-year future restatement rates are 0.366 and 0.246, respectively; the difference is significant at $p < 0.01$). Moreover, our previous results suggest that reported control weaknesses can increase the likelihood of lawsuits and regulatory actions in the event of a restatement, which could also influence auditors' risk assessments. We thank an anonymous reviewer for suggesting this possibility.

TABLE 5

Auditor Turnover Regressions

		Dependent Variable: *AUD TURN*				Auditor Dismissals Only		Auditor Resignations Only			
		Full Sample		Propensity Score-Matched Sample		Full Sample		Full Sample		Constrained Sample	
Parameter	Pred. Sign	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect
Intercept	+/–	–0.579		–1.238		–0.853**		–1.240		–0.725	
		(0.389)		(0.947)		(0.408)		(0.833)		(0.927)	
REPORT ICW	+/–	0.309*	0.055	0.668**	0.093	0.176	0.026	0.705**	0.035	0.605*	0.040
		(0.174)		(0.329)		(0.189)		(0.307)		(0.318)	
REST MAGNITUDE	–	0.183	0.000	–0.756	–0.002	–0.270	0.000	0.829	0.001	1.000	0.001
		(1.589)		(3.164)		(1.658)		(2.830)		(2.560)	
IRREGULARITY	+	0.336**	0.050	–0.041	–0.006	0.214	0.028	0.794**	0.022	0.514	0.022
		(0.178)		(0.353)		(0.183)		(0.465)		(0.525)	
REST REVENUE	+	0.000	0.000	0.162	0.025	0.112	0.016	–0.577	–0.016	–0.629	–0.026
		(0.205)		(0.409)		(0.211)		(0.503)		(0.503)	
REST COUNT	+	0.068*	0.022	0.198**	0.053	0.018	0.005	0.207***	0.013	0.183***	0.017
		(0.045)		(0.108)		(0.050)		(0.077)		(0.078)	
REST YEARS	+	–0.030	–0.013	0.028	0.012	–0.021	–0.008	–0.053	–0.005	–0.071	–0.011
		(0.034)		(0.057)		(0.035)		(0.078)		(0.081)	
CAR	–	–0.449	–0.004	1.204	0.010	–0.622	–0.005	0.745	0.002	1.312	0.004
		(0.964)		(1.976)		(1.002)		(2.111)		(2.366)	
PREVIOUS RETURN	–	–0.036	–0.003	–0.627*	–0.049	–0.069	–0.005	0.235	0.004	0.275	0.007
		(0.168)		(0.434)		(0.180)		(0.306)		(0.332)	
SIZE	–	–0.143**	–0.042	–0.143	–0.030	–0.103*	–0.026	–0.291**	–0.017	–0.280**	–0.026
		(0.062)		(0.164)		(0.064)		(0.150)		(0.161)	
BIG4	–	–0.268*	–0.048	–0.499*	–0.083	–0.156	–0.023	–0.424	–0.019	–0.489*	–0.034
		(0.206)		(0.364)		(0.224)		(0.341)		(0.370)	
SALES GROWTH	+	0.181	0.004	0.699	0.019	–0.004	–0.001	0.796*	0.005	0.838*	0.008
		(0.302)		(0.628)		(0.325)		(0.514)		(0.546)	

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TABLE 5 (continued)

		Dependent Variable: *AUD TURN*				Auditor Dismissals Only		Auditor Resignations Only			
		Full Sample		Propensity Score-Matched Sample		Full Sample		Full Sample		Constrained Sample	
Parameter	Pred. Sign	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect	Est. Coeff. (Std. Error)	Marg. Effect
Observations		601		174		601		601		388	
Likelihood ratio Chi-squared		24.87		18.92		10.74		28.11		22.39	
Pseudo R^2		0.065		0.170		0.033		0.239		0.209	

***, **, * Denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs, and two-tailed otherwise.
This table presents the results of auditor turnover regressions using probit estimation. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of turnover occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values. The samples exclude observations with auditor changes during the year preceding the restatement.
Variables are as defined in Appendix A.

VI. ADDITIONAL ANALYSES AND SENSITIVITY TESTS

Penalty Timing and the Associations between Penalties

We next consider the associations between the various penalties in our analyses by examining their timing and whether certain penalties tend to precede or follow other penalties. Underlying this analysis is the question of whether the penalties are interrelated in such a way that perhaps one penalty is driving the others (e.g., does the SEC simply pile on with an AAER after observing that investors have filed a class action lawsuit?).

In Table 6, Panel A we tabulate distributional statistics on the time to enforcement (i.e., days between the initial restatement announcement and the subsequent penalty) for the various penalties. Class action lawsuits tend to be filed fairly quickly in the aftermath of a restatement (median of 40 days; roughly a quarter occur within a week). AAERs, by contrast, tend to be issued much later (median of 862 days, which is roughly 2.4 years).[26] Management and auditor turnovers tend to occur later than lawsuits, but earlier than AAERs (median times of 145 days and 135 days, respectively).

In Panel B of Table 6 we provide information on the relative timing for pairs of penalties. With the exception of AAERs, Panel B indicates that the vast majority of the penalties in our sample are not preceded by any of the other penalties. For AAERs, roughly half are preceded by lawsuits. AAERs are preceded by management turnover about half the time as well.

We next reestimate our main regression models after including additional controls for the occurrence of other, earlier penalties. For example, for the AAER regression, we include control variables indicating whether the AAERs were preceded by the filing of a class action lawsuit, management turnover, or auditor turnover. The results of these regressions are reported in Table 6, Panel C (for brevity, we tabulate only the results for the *REPORT ICW* variable and summary statistics, but the models also include the full sets of corresponding controls, as in our main regressions). Consistent with the conclusions from Panel B, the additional controls for other penalties are generally insignificant (untabulated), with the exception of the AAER regression, where the controls for prior litigation and prior management turnover are both positive and significant. More importantly, the inclusion of the additional controls does not alter our inferences with respect to *REPORT ICW;* it remains positive and significant in the litigation, management turnover, and auditor turnover models, and insignificant in the AAER model.[27]

As a whole, the results in Table 6 indicate that while there are some associations between the penalties we examine, the overlap is limited enough that no one penalty appears to be driven by the others.[28]

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[26] Our search for AAERs and lawsuits was conducted through the end of 2013. Thus, we have at least three years after each restatement in our sample in which to observe AAERs and lawsuits. Because AAERs are occasionally issued more than three years after restatements, we conduct two additional sensitivity analyses. First, we reestimate our AAER model using only pre-2009 restatements, thus ensuring at least five years in which to observe an AAER. Second, we adjust the coding of our *AAER* variable such that it is only set equal to 1 if the firm receives an AAER within three years of its restatement. Our inferences are unaffected in both cases.

[27] We also reestimate the AAER model with the additional controls using the propensity score-matched sample (the only specification from Table 2 in which *REPORT ICW* is statistically significant) in an untabulated test. Similar to Table 2, *REPORT ICW* is positive and statistically significant in this specification, which reinforces the conclusion that controlling for the other penalties has little impact on our results.

[28] To investigate the possibility that consequences differ with the extent of non-reporting, we also decompose the *REPORT ICW* = 0 category into the four groups described in Section IV based on how control weaknesses are discussed in firms' post-restatement disclosures (acknowledge; acknowledge, but claim to now be remediated; deny; no mention). We then reestimate our main penalty models for these *REPORT ICW* = 0 firms with "acknowledge" as the reference group and indicator variables for the other groups. These indicators are generally insignificant, with the exception of "no mention," which is negative in the litigation and auditor turnover models.

TABLE 6

Penalty Timing and Associations between Penalties

Panel A: Time to Enforcement (Days)

	Mean	Q1	Median	Q3
AAER	882	573	862	1,186
LITIGATION	119	8	40	138
MGT TURN	165	80	145	252
AUD TURN	160	64	135	253

Panel B: Proportion of Penalties Preceded by Other Penalties

	Proportion Preceded by:			
	AAER	*LITIGATION*	*MGT TURN*	*AUD TURN*
AAER		51.1%	55.8%	4.7%
LITIGATION	1.5%		18.5%	3.1%
MGT TURN	0.5%	10.8%		6.2%
AUD TURN	0.0%	4.7%	17.2%	

Panel C: Regressions with Additional Controls for Other, Earlier Penalties

	AAER Est. Coeff. (Std. Error) [Marg. Effect]	*LIT EXCL DISMISS* Est. Coeff. (Std. Error) [Marg. Effect]	*MGT TURN* Est. Coeff. (Std. Error) [Marg. Effect]	*AUD TURN* Est. Coeff. (Std. Error) [Marg. Effect]
REPORT ICW	–0.105	0.477**	0.476***	0.369**
	(0.251)	(0.243)	(0.164)	(0.179)
	[–0.009]	[0.046]	[0.157]	[0.066]
Observations	659	659	487	601
Likelihood ratio Chi-squared	109.9	99.41	50.3	31.87
Pseudo R^2	0.346	0.330	0.088	0.083

***, **, * Denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests.
Panel A presents distributional statistics on the time to enforcement (i.e., days between the initial restatement announcement and the subsequent penalty) for the various penalties. Panel B presents information on the relative timing for pairs of penalties. Panel C presents the results of reestimating the various penalty regressions after including additional controls for the occurrence of other, earlier penalties. Those regressions also include the full set of control variables as in the primary specifications (results for control variables are untabulated for brevity). Reported marginal effects represent the average change in probability of the dependent variable being equal to 1 for a change in *REPORT ICW* from 0 to 1. The *MGT TURN* column excludes observations with management turnover (either CEO or CFO) during the year preceding the restatement. The *AUD TURN* column excludes observations with auditor changes during the year preceding the restatement.
Variables are as defined in Appendix A.

Endogeneity due to Unobservables

A limitation of propensity score-matching as a treatment for endogeneity is that it only captures the observable factors included in the first-stage matching model. We also consider here the potential for endogeneity due to omitted factors (unobservables). In our setting, it is possible that

unmodeled differences between *REPORT ICW* = 1 firms and *REPORT ICW* = 0 firms exist and that these differences are also correlated with the penalties we examine. We address this possibility by estimating each penalty model jointly with the *REPORT ICW* determinants model from Rice and Weber (2012) using bivariate probit estimation (Greene 2003).[29] This approach considers the correlation between the error terms of the simultaneously estimated models, which addresses the possibility of endogeneity due to unobservables. The general setup is:

$$y_1{}^* = \boldsymbol{\beta_1 x_1} + \gamma_1 y_2 + \varepsilon_1, \quad y_1 = 1,\ (y_1{}^* > 0), \quad \text{(Model 1)}$$
$$y_2{}^* = \boldsymbol{\beta_2 x_2} + \varepsilon_2, \quad y_2 = 1,\ (y_2{}^* > 0), \quad \text{(Model 2)}$$

where ε_1 and ε_2 are normally distributed error terms. Model 1 represents our various penalties models (i.e., such that the dependent variable is either *AAER, LITIGATION, MGT TURN,* or *AUD TURN*) and Model 2 represents the Rice and Weber (2012) model of determinants of *REPORT ICW*. The question is whether the errors from Model 1 are correlated with the errors from Model 2, where ρ represents the correlation parameter. If the errors are not correlated, then this suggests a low likelihood of omitted correlated variables, which would be captured in the error term of each model (Greene 1998). We use bivariate probit estimation, which estimates ρ using maximum likelihood, to test the null hypothesis of no correlation ($\rho = 0$).

Diagnostics from untabulated bivariate probit estimations reveal that for each of the four penalties, the correlation between the error terms from the *REPORT ICW* model and the error terms from the respective penalty model is insignificantly different from zero. Thus, the null hypotheses of $\rho = 0$ is not rejected in any of the cases, and single-equation estimation, as in our primary results, is preferred (Greene 1998). While we cannot completely rule out the possibility, these tests indicate that endogeneity is unlikely to be a significant concern.

Irregularities Restatements

Our primary results suggest a lack of penalties for SOX 404 reporting failures. Those failures, however, could stem from either failures to detect the weaknesses (i.e., unintentional oversights or misjudgments about materiality) or purposeful failures to report known weaknesses (i.e., intentional misreporting). Thus, a potential explanation for the lack of penalties we observe is that it is driven by unintentional failures to detect weaknesses. Put differently, if managers fail to report material weaknesses simply because they do not know about them, then it is possible that their actions may be viewed as less egregious and associated penalties unlikely.

We investigate this possibility by confining the sample to only those restatements attributed to irregularities. Irregularities represent "intentional misreporting" in the financial statements (Hennes et al. 2008). Rice and Weber (2012) argue that managers making such intentional misstatements in the financial statements are likely to be aware of the control weaknesses they are exploiting to do so. As such, irregularities restatements provide a subsample for which SOX 404 reporting failures are less likely to be the result of unintentional oversights.[30] If there are penalties specifically for intentional misreporting of internal control effectiveness, then we are more likely to observe them for the irregularities subsample.

In untabulated analysis, we reestimate our main models after narrowing the sample to only irregularities restatements. Consistent with our main results, the estimated coefficient on *REPORT*

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29 Because each of our penalty variables is binary, as is the potentially endogenous variable of interest (*REPORT ICW*), bivariate probit estimation is more appropriate in our setting than the common two-stage Inverse Mills Ratio approach, which requires the second stage to be a linear model (e.g., Tucker 2010).

30 We acknowledge that intent is not observable and that irregularities are merely a proxy. Thus, while unintentional oversights are less likely in this subsample, we cannot rule out that they are still possible.

ICW is insignificant in the AAER model, but is positive and significant when both *LIT EXCL DISMISS* and *MGT TURN* are the dependent variables, with marginal effects of 6 and 13 percent, respectively. The estimated coefficient on *REPORT ICW* is insignificant for auditor turnover overall, but is positive and significant with a marginal effect of 6 percent for auditor resignations. Thus, even for this subsample where there is a higher likelihood of intentional misreporting, we again find no evidence of enforcement of SOX 404. Instead, penalties are more likely for restating firms that have previously reported control weaknesses.

Monetary Penalties

Finally, we examine the amounts of monetary penalties for AAERs and class action lawsuits. While our primary results suggest that the *occurrence* of penalties is more likely for firms that previously reported their control weaknesses, it is possible that the *amounts* of the associated penalties may be lower if, for example, managers are more likely to be perceived as acting in good faith when they report control weaknesses. This does not appear to be the case. In untabulated analysis, we find that the monetary amounts associated with AAERs and class action settlements are similar across *REPORT ICW* groups. For AAERs, the median amounts are \$1,000,000 for *REPORT ICW* = 0 firms and \$952,000 for *REPORT ICW* = 1 firms. The median settlements for class action lawsuits are \$11,000,000 and \$10,000,000, respectively.[31] Thus, there is no evidence that SOX 404 is enforced through the incurrence of larger monetary penalty amounts.

VII. SUMMARY AND CONCLUSION

In this paper, we study a sample of firms that are subject to internal control reporting under SOX 404 and have restatements. Only a minority of these firms report their control weaknesses in a timely manner, as required by SOX 404; the majority acknowledge their weaknesses only after having announced the need for a related restatement. We investigate whether penalties surrounding the restatements differ for these two groups of firms and, in doing so, provide evidence on the consequences of failing to comply with SOX 404.

We examine several potential enforcement mechanisms for SOX 404, including both public mechanisms (SEC sanctions) and private mechanisms (class action lawsuits, top management turnover, and auditor turnover). We find no evidence that penalties following a restatement are more likely for firms that fail to detect and disclose their control weaknesses as required. Instead, firms that do report their control weaknesses in a timely manner are generally more likely to face each of these penalties in the event of a later restatement. These results are consistent with the disclosure of control weaknesses making it difficult for management to plausibly claim later that they had been unaware of the underlying conditions in the control environment that led to their restatements.

Taken as a whole, our results, along with those of Hogan et al. (2013), suggest that the enforcement mechanisms surrounding SOX 404 are unlikely to provide strong incentives to detect and disclose existing control weaknesses. In some cases, they may even create perverse incentives to avoid reporting control weaknesses until their revelation is forced by a restatement. These results offer a potential explanation for why the majority of restatements occur at firms that previously claimed that their internal controls were effective. The very nature of control reporting, which involves a high degree of judgment and the inherent difficulty of assessing the effectiveness of processes, is likely to make stringent enforcement a challenge. However, from a public policy

[31] None of these differences are statistically significant. In untabulated analysis, we also estimate regression models with monetary penalties as dependent variables and controls for firm size and damages. *REPORT ICW* is insignificant in all cases.

perspective, our results suggest that in the long run, without more stringent enforcement, SOX 404 may be unlikely to fulfill its underlying objective of enhancing investor confidence in the reliability of financial reporting, particularly if control weaknesses continue to be acknowledged only after the restatements they helped create.

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APPENDIX A

Variable Definitions

Variable	Definition
REPORT ICW	1 if the firm reported the existence of a material weakness in internal control over financial reporting in any SOX 404 report during their misstatement period, and 0 otherwise (source: Audit Analytics).
AAER	1 if the firm was subject to an Accounting and Auditing Enforcement Release from the SEC related to the restatement, and 0 otherwise (source: SEC website at: http://www.sec.gov).
LITIGATION	1 if a class action lawsuit related to the restatement was filed against the firm, and 0 otherwise (source: Stanford Securities Class Action Clearinghouse).
LIT EXCL DISMISS	1 if a class action lawsuit related to the restatement was filed against the firm and was not later dismissed, and 0 otherwise (source: Stanford Securities Class Action Clearinghouse).
MGT TURN	1 if the firm had a CEO or CFO change during the one-year period following the restatement announcement, and 0 otherwise (source: Audit Analytics).
AUD TURN	1 if the firm had an auditor change during the one-year period following the restatement announcement, and 0 otherwise (source: Audit Analytics).
REST MAGNITUDE	The cumulative change in reported earnings due to the restatement, scaled by total market value of common equity at the end of the misstatement period, winsorized at the 1st and 99th percentiles (source: Audit Analytics).
IRREGULARITY	1 for those restatements associated with fraud or where SEC or board-instigated independent investigations occur, and 0 otherwise (source: Audit Analytics).
REST REVENUE	1 if the restatement involves revenue recognition, and 0 otherwise (source: Audit Analytics).
REST COUNT	Number of distinct account types being restated (source: Audit Analytics).

(continued on next page)

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APPENDIX A (continued)

Variable	Definition
REST YEARS	Length of the misstatement period in years (source: Audit Analytics).
CAR	Cumulative abnormal return over the days (0, +1) relative to the restatement announcement date, calculated as the raw stock return minus the CRSP equally weighted market portfolio return (source: CRSP).
LIT INDUSTRY	1 if the firm is in the biotech (SIC codes 2833–2836 and 8731–8734), computer (3570–3577 and 7370–7374), electronics (3600–3674), or retail (5200–5961) industry, and 0 otherwise (source: Compustat).
PREVIOUS RETURN	Buy-and-hold abnormal return, based on the CRSP equally weighted market portfolio return, over the window (−252, −2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
RETURN STD DEV	Standard deviation of daily stock returns over the window (−252, −2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
RETURN SKEWNESS	Skewness of daily stock returns over the window (−252, −2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
SHARE TURNOVER	Probability that a share was traded within a given time period, calculated as: $[1 - \Pi_t(1 - \textit{shares traded}_t/\textit{total shares}_t)]$, accumulated over the one-year period ending on the second day prior to the restatement announcement (source: CRSP).
SIZE	Natural log of total market value of common equity at the end of the misstatement period, winsorized at the 1st and 99th percentiles (source: Compustat).
BIG4	1 if the firm received a SOX 404 audit opinion from Deloitte, PricewaterhouseCoopers, Ernst & Young, or KPMG during the misstatement period, and 0 otherwise (source: Audit Analytics).
SALES GROWTH	Growth in sales over the last year of the misstatement period (source: Compustat).
ROA	Return on assets for the year prior to the restatement, calculated as operating income before interest and taxes divided by total assets (source: Compustat).
DAMAGES	The firm's market capitalization at its highest point during the misstatement period, minus its market capitalization on the day after the restatement announcement (or, if unavailable, on the day of the restatement announcement), truncated at zero (source: CRSP).

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Admitting Mistakes: Home Country Effect on the Reliability of Restatement Reporting

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ABSTRACT: We study the frequency of restatements by foreign firms listed on U.S. exchanges. We find that the restatement rate of U.S.-listed foreign firms is significantly lower than that of comparable U.S. firms and that the difference depends on the firm's home country characteristics. Foreign firms from countries with a weak rule of law are less likely to restate than are firms from strong rule of law countries. While the lower rate of restatements can represent an absence of errors, it can also indicate a lack of detection and disclosure of errors and irregularities. We infer the effect of detection and disclosure by associating the frequency of restatements with the quality of the firm's internal control system. We find that only U.S. firms and foreign firms from strong rule of law countries show a positive association between restatement frequency and internal control weaknesses. Firms from weak rule of law countries show no significant association. We interpret these findings as home country enforcement affecting firms' likelihood of detecting and reporting existing accounting misstatements. This suggests that for U.S.-listed foreign firms, less frequent restatements can be a signal of opportunistic reporting rather than a lack of accounting errors and irregularities.

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I. INTRODUCTION

We examine the reporting of accounting restatements by foreign firms listed in the United States. Accounting rules in the U.S. require firms to issue a restatement correcting prior material errors upon discovery (Financial Accounting Standards Board [FASB] 2005, Accounting Standards Codification [ASC] 250).[1] Timely detection and reporting of accounting errors and irregularities ensure that a firm's reported financials are free of any misstatements. Without enforcement that ensures the prudent correction of existing misstatements, there will likely be systematic underreporting of restatements, which will allow "bad" type firms to pool with "good" type firms and possibly lower investors' faith in the reported financials.[2] Therefore, understanding the determinants of reporting restatements is important to better assess the reliability of reported financials.

The mandatory reporting requirement for restatements implies that the likelihood of an accounting restatement should increase with the existence of accounting errors or irregularities. While more frequent restatements implies more errors or irregularities, it also suggests the presence of internal controls that led to timely detection and disclosure of the misstatement. This is because reporting a restatement involves two steps. First, managers commit an unintentional error or deliberate manipulation that results in misstated accounting numbers. Second, the firm (or its auditor) detects and reports the misstatement (Dyck, Morse, and Zingales 2010; Keune and Johnstone 2012). The second step—the detection and self-reporting of the misstatement—depends on the firm's and auditor's ability and willingness to comply with reporting rules (Heitzman, Wasley, and Zimmerman 2010). Therefore, observing a restatement is a joint outcome of (1) committing an accounting error or irregularity, and (2) detecting and reporting the misstatement. This two-step process implies that a lower rate of restatements indicates fewer accounting mistakes only if there is timely detection and reporting of misstatements.

Many prior studies focus on the first step, showing how restatements are associated with proxies of accounting errors and irregularities (Richardson, Tuna, and Wu 2002; Doyle, Ge, and McVay 2007). In this study, we explicitly consider the second step, which implies that a higher frequency of restatements also suggests better detection and reporting of misstatements.

We use the large number of restatements in recent years by both U.S. and foreign firms listed in the U.S. to examine the reliability of restatement reporting in a cross country setting.[3] The self-reported nature of restatements provides a good setting to assess how home country characteristics influence the financial reporting of foreign firms listed in the U.S. Further, since foreign firms are subject to the disclosure requirements set forth by the Securities and Exchange Commission (SEC), this setting allows us to examine the effect of home country characteristics on the financial reporting

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[1] FASB (2005) ASC Topic 250, *Accounting Changes and Error Corrections*, states, "Any error in the financial statements of a prior period discovered after the financial statements are issued shall be reported as an error correction, by restating the prior-period financial statements." Also, ASC 105 notes that the provisions of GAAP apply only to material items.

[2] By "good" type, we refer to firms that practice high-quality financial reporting, which is less prone to errors and irregularities. "Bad" type firms are those that are more likely to have errors and irregularities, but that are unlikely to detect or disclose the misstatements.

[3] We define reliable restatement reporting as detecting and reporting accounting problems (both intentional and unintentional) in accordance with ASC 250. Reliable restatement reporting ensures that the lack of restatements is indeed indicative of an absence of accounting errors or earnings management.

of foreign registrants while generally holding the extent of U.S. regulation constant (Jenkins 1999; Lang, Raedy, and Wilson 2006).

In particular, we examine whether restatement reporting varies by country-level factors that influence how firms comply with the restatement reporting rules. Following prior literature (Ball, Kothari, and Robin 2000), we argue that a company's home country shapes its reporting behavior and that this effect continues even after listing in the U.S. Lang et al. (2006) document more earnings management in foreign listers compared to U.S. firms, which suggests a higher likelihood of restatements by foreign listers relative to U.S. firms, assuming that misstatements are detected and reported equally for foreign and U.S. firms. However, if foreign firms fail to report misstatements, due to non-detection or opportunistic reporting, then there may be no significant relation between earnings management and the rate of restatements.

Our sample comprises 7,453 firm-year observations for U.S.-listed foreign firms from 51 countries between 2000 and 2010. Foreign firms report accounting restatements in 4.7 percent of firm-years, compared to 7.3 percent for a matched sample of U.S. firms.[4] We confirm the lower rate of restatements for foreign firms compared to U.S. firms in multivariate tests that control for factors that prior studies have found to be associated with restatements. The coefficient estimate suggests that foreign firms are 46 percent less likely to restate compared to the U.S. matched sample.

Next, we examine whether home country factors affect the likelihood of restatements. We follow prior papers such as Ball et al. (2000) and Leuz, Nanda, and Wysocki (2003), which document cross-country variation in accounting quality driven by the strength of domestic legal institutions. We use a country-level measure of the rule of law as a summary indicator of the extent of compliance with laws and regulations that can shape a firm's reporting behavior by impacting factors such as auditor effort, investor protection, and managerial self-dealing, among others. Empirically, we use the rule of law index from the World Bank's Worldwide Governance Indicators (Kaufmann, Kraay, and Mastruzzi 2003) used in La Porta, Lopez-de-Silanes, and Shleifer (2006).[5]

We find that the frequency of restatements varies with the home country's rule of law. Firms from weak rule of law countries are less likely to restate, with 4.2 percent of firms restating, compared to 7.5 percent for the matched sample of U.S. firms (p-value < 0.001). On the other hand, firms from strong rule of law countries show a smaller difference in their restatement frequency compared to matched U.S. firms (5.0 percent versus 7.2 percent of firm-years, p-value $<$ 0.001). The findings hold after distinguishing foreign firms that provide generally accepted accounting principles (GAAP) reconciliation versus those using U.S. GAAP itself. In economic terms, after controlling for other determinants of restatement probability, firms from weak rule of law countries are 42 percent less likely to restate compared to firms from strong rule of law countries.

Fewer restatements from weak rule of law countries can represent an absence of accounting misstatements, as well as a lack of detection and disclosure. We distinguish between the two interpretations by relating the frequency of restatements with the quality of the firm's internal control (IC) system, measured as the extent of material weaknesses (MW) in its internal controls over financial reporting (ICFR). Weak internal controls indicate that the firm has a less robust

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[4] The restatements we consider are all made to correct misstatements resulting from a failure to comply with U.S. reporting standards. We do not measure violations of local accounting rules since we are interested in understanding reporting behavior in the U.S., how it compares to the reporting behavior of similar U.S. firms, and how it varies across countries. Hence, we use U.S. reporting requirements as a common basis.

[5] Rule of law measures the extent to which agents have confidence in and abide by the rules of society. These include the effectiveness and predictability of the judicial system, the enforceability of contracts, and perceptions about the incidence of crime in the country (La Porta, Lopez-de-Silanes, and Shleifer 2006) as measured in the year 2000.

reporting system, which increases the possibility of accounting errors, both intentional and unintentional. Thus, if firms correctly report their accounting misstatements, then the frequency of restatements will be *positively* associated with the firm's internal control material weaknesses (ICMW). In contrast, if accounting errors go undetected or unreported, then the relationship between restatement frequency and ICMW will weaken. We infer the quality of detection and disclosure by examining the sensitivity of the restatement rate to the effectiveness of the firm's internal control system.[6]

We find that the association between restatement frequency and ICMW increases with the home country's rule of law effectiveness. Firms from weak rule of law countries show no evidence of more frequent restatements when there are more ICMW. In contrast, firms from strong rule of law countries and the matched U.S. sample show the expected positive relationship between restatements and ICMW. This suggests that the lower frequency of restatements in weak rule of law countries is due to weaker compliance with restatement reporting, rather than an absence of accounting misstatements.[7]

Foreign firms may restate less if they avoid restating minor errors, but report all severe accounting irregularities. We examine this possibility by differentiating restatements involving errors from those with likely accounting irregularities (Palmrose, Richardson, and Scholz 2004; Hennes et al. 2008). We test whether the lower rate is observed for restatements pertaining to accounting irregularities, as well as those related to minor errors. We find that foreign firms, especially those from weak rule of law countries, are less likely to report accounting irregularity restatements than comparable U.S. firms. Also, using earnings management (EM) proxies as a predictor of restatements from accounting irregularities, we find that the sensitivity of EM to accounting irregularity restatements is positive and significant only for U.S. firms and foreign firms from strong rule of law countries. For foreign firms from weak rule of law countries, we find no relation between EM and the likelihood of accounting irregularity restatements. This suggests that avoidance of restatement is not limited to errors; it exists even for accounting irregularities.

Our study contributes to a few streams of literature. Articles that examine the causes and consequences of restatements generally focus on U.S. firms and conclude that restatements represent poor earnings quality and that firms suffer capital market consequences as a result (Palmrose et al. 2004; Plumlee and Yohn 2010). We highlight two stages in the restatement decision, and show that lower restatement rates may indicate (1) a lower incident of accounting errors or irregularities, as well as (2) the lax detection and disclosure of existing misstatements. The implication of the two-step restatement reporting process is that fewer restatements do not necessarily imply higher (or lower) financial reporting quality. Our study highlights that a positive relation between restatements and financial reporting quality depends on the reliable detection and disclosure of misstatements.

Next, our findings have implications for understanding the reporting quality of foreign firms listed in the U.S. Stringent disclosure rules and the resultant transparency serve as important mechanisms by which foreign firms bond to the U.S. regulatory regime. The lower earnings quality found in Lang et al. (2006) suggests greater errors and irregularities in the financial statements of

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[6] Another predictor of restatements is the level of earnings management (EM) measured using accrual-based models. Such models yield measures of discretionary accruals, which are likely to primarily capture intentional accounting irregularities and not unintentional accounting errors. In additional analysis, we limit our restatement sample to those that are more likely to result from intentional accounting irregularities, following Hennes, Leone, and Miller (2008), and use EM as a predictor of such restatements.

[7] We note that ICMW may also be subject to reporting discretion (Gong, Ke, and Yu 2013). More importantly, if factors that affect reporting discretion in ICMW also affect restatement reporting, then our findings may be subject to a systematic measurement error. We provide additional tests to account for this measurement error (see Section IV).

foreign listers. Despite this, we find that foreign firms are less likely to restate, a finding that has implications for investors and regulators. Prior studies show that investors benefit in a regime that offers effective correction of misstatements, since reliable information promotes better resource allocation (Kedia and Philippon 2009; Beatty, Liao, and Yu 2013).[8] Failure to correct misstatements results in the pooling of good and bad type firms, and such pooling makes it difficult for investors to sort firms based on their economic performance.

Relatedly, the lack of restatements in the presence of errors and irregularities implies fewer *ex post* penalties, which reduces *ex ante* discipline in financial reporting. Fewer restatements lowers investors' ability to hold managers and auditors accountable for poor financial reporting through CEO turnover or securities litigation, since restatements are a major trigger for both these mechanisms (Johnson, Nelson, and Pritchard 2007; Hennes et al. 2008).[9] Lawsuits accompanying restatements are less likely to be dismissed and more likely to be settled in favor of the plaintiff (Johnson et al. 2007) and for larger amounts (Brochet and Srinivasan 2013) than lawsuits without restatements. Cheng, Srinivasan, and Yu (2013) show that foreign listers are less likely to be sued in securities lawsuits compared to U.S. firms, in part due to fewer restatement triggers. Firms that do not correct misstatements face lower risk of SEC action since restatements prompt SEC investigations and investor scrutiny (Karpoff, Lee, and Martin 2008; Files, Swanson, and Tse 2009). Therefore, our results imply that U.S.-listed foreign firms may be under-scrutinized by U.S. public and private enforcement mechanisms. Our findings suggest that companies from countries with weaker domestic rule of law (RoL) are a potential focus area for investors and regulators (e.g., SEC, Public Company Accounting Oversight Board [PCAOB]) to better identify firms with opportunistic restatement behavior.

The remainder of the paper is organized as follows. Section II reviews the literature and develops our hypotheses. Section III describes the data and empirical tests; Section IV presents our results. In Section V, we present additional analyses, and we conclude in Section VI.

II. HYPOTHESIS DEVELOPMENT AND INSTITUTIONAL DETAILS

Home Country Effect and Reporting by Foreign Firms Listed in the U.S.

Foreign firms listed in the U.S. follow financial reporting requirements set forth by the SEC and relevant laws such as the Securities Exchange Act of 1934 and the Sarbanes-Oxley Act of 2002 (SOX). These companies are required to make ongoing filings with the SEC and are subject to SEC oversight. Prior research considers this commitment to ongoing disclosure and the enforcement of securities laws to be among the benefits of listing in the U.S. (Karolyi 2006). In addition to subjecting firms to generally higher-quality reporting standards, U.S. listing can increase reporting quality by stricter monitoring of auditors. Auditors of firms listed in the U.S. face higher litigation risk than those in other countries (La Porta et al. 2006; Choi, Kim, Liu, and Simunic 2009), and monitoring by the Public Company Accounting Oversight Board (PCAOB), which can lead to a

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[8] Palmrose and Scholz (2004) describe the timely correction of inaccurate disclosures as a mechanism devised under the Securities Acts to ensure that investors possess accurate information for resource allocation decisions. Kedia and Philippon (2009) find that misreporting has real resource allocation effects. They show that low-productivity firms hire and invest too much and distort their performance with poor accounting. When misreporting is detected, firms shed labor and capital, improving productivity. Bushman and Smith (2001, 294) argue that "managers can identify promising new investment opportunities on the basis of the high profit margins reported by other firms." Beatty et al. (2013) suggest that companies distort their investment behavior based on industry peers' overstated earnings.

[9] Restatements are often followed by disciplinary managerial and board turnover (Desai, Hogan, and Wilkins 2006; Srinivasan 2005) and auditor turnover (Hennes et al. 2014). Companies also make governance and disclosure improvements to regain reputation (Chakravarthy, DeHaan, and Rajgopal 2014).

superior audit effort.[10] Case law shows that provisions of the securities laws extend to all auditors of U.S. registrants, even if the auditors are not U.S.-based (Seetharaman, Gul, and Lynn 2002).

Despite such monitoring, prior studies find that the quality of disclosure by U.S.-listed foreign firms is not on par with that of U.S. firms. Lang et al. (2006) find that earnings of foreign issuers show more evidence of earnings management than earnings of U.S. firms. They also find that the accounting quality of cross-listed firms varies systematically by home country characteristics such as investor protection and legal enforcement. Foreign firms from weak investor protection countries are less likely to voluntarily report incidents of internal control weaknesses (Gong et al. 2013) and to provide management forecasts (Hope, Kang, and Kim 2013). These findings suggest that U.S. regulation, monitoring by the SEC, and the demands of U.S. investors do not completely harmonize the disclosure quality of U.S.-listed foreign firms with that of U.S. firms.

There are reasons to expect the restatement rate of U.S.-listed foreign firms to differ from that of U.S. firms. Restatements correct both unintentional errors and intentional accounting irregularities. Plumlee and Yohn (2010) find that the majority of the restatements in the U.S. arise from unintentional errors. To the extent that U.S.-listed foreign firms incur more errors from misapplying GAAP, they can have more restatements. Also, Lang et al. (2006) show that foreign firms listed in the U.S. show more earnings management compared to similar U.S. companies. If earnings management proxies capture the level of accounting irregularities, then one would expect financial statements of foreign firms to show a higher rate of restatements.

The prediction that more accounting errors and irregularities will lead to higher restatement rates assumes that most errors and irregularities are detected and reported. However, firms can have incentives to avoid correcting misstatements because truthful reporting of misstatements, whether intentional or unintentional ones, will draw investors' attention and undermine the credibility of financial statements (Palmrose and Scholz 2004; Collins, Masli, Reitenga, and Sanchez 2009; DeHaan, Hodge, and Shevlin 2012). U.S.-listed foreign firms can be sensitive to such incentives, as one reason for listing in the U.S. is to signal high quality by bonding to a stricter financial reporting regime (Coffee 2002). If such foreign firms are less likely to detect and report existing errors and irregularities, then it is possible that the restatements rates may be lower for U.S.-listed foreign firms compared to U.S. firms.

Prior research suggests that the extent of bonding to the U.S. regulatory and governance regime differs systematically across countries (Frost and Pownall 1994). There are differences across countries in the domestic supply of expert intermediaries such as auditors, analysts, lawyers, and institutional investors, and in the extent of enforcement by local capital market regulators. In fact, enforcement by the SEC and private litigation also relies on local infrastructure (e.g., lawyers and auditors) to support enquiries and action in the home country. Consistent with this, Leuz et al. (2003) find that foreign firms exhibit more evidence of earnings management, especially in countries with weak enforcement.[11] If U.S.-listed foreign firms continue to engage in more earnings management, and even more so when they are from weak rule of law countries, then the likelihood of restatements for firms from these countries will be higher. However, if U.S.-listed foreign firms are less likely to detect and report existing misstatements, then these firms may exhibit a lower rate of restatements relative to U.S. firms, despite higher earnings management levels. We initially

[10] For example, for foreign registrants, the quality control standards of PCAOB (SECPS 1000.08) in Appendix K require a qualified auditor familiar with SEC rules and regulations ("filing reviewer") to review the sample audit procedure of all non-U.S. auditors.

[11] While prior studies suggest that there are significant differences in reporting behavior across countries, country-level factors need not play any role in the reporting quality of U.S.-listed foreign companies if high-quality firms are able to overcome weak country-level institutions when they cross-list. Alternately, lower-quality firms can continue to show opportunism even when they are from a strong rule of law country.

examine whether the restatement rate of U.S.-listed foreign firms differs from that of U.S. firms, and then test H1 as to whether the difference varies by home country characteristics:

H1: The probability of restatements by foreign firms cross-listed in the U.S. will vary by the level of home country rule of law.

We use the measure "rule of law" (RoL) in the home country from La Porta et al. (2006) as a summary measure to capture the variation across countries on all of the dimensions discussed above. We believe this parsimony to be desirable and necessary, as many of the local institutional development measures are highly endogenous. This measure has been widely used in the prior literature (e.g., Doidge, Karolyi, and Stulz 2007). We also confirm the robustness of the results with the alternate measure of the RoL index used in Leuz et al. (2003).[12]

Simply comparing the frequency of restatements may indicate (1) a higher occurrence of accounting problems, as well as (2) the prudent detection and disclosure of existing errors. Because the purpose of our study is to highlight the second step—the detection and disclosure of errors—we examine the sensitivity of restatement rates to the effectiveness of a firm's ICs, a firm characteristic that is negatively related to the extent of the accounting problems. That is, in addition to comparing the *levels* of restatement frequency, we examine the *sensitivity* of restatements to the presence of ICMW to infer the detection and reporting of existing errors.

We infer the level of discretion in restatement reporting using the association between restatements and ICMW. Prior studies demonstrate a link between the quality of the firm's internal controls over financial reporting and the likelihood of subsequent accounting restatements (Hammersley, Myers, and Shakespeare 2008; Plumlee and Yohn 2010). Firms with weak internal control systems show a higher likelihood of accounting misstatements (DeFond and Jiambalvo 1991). Thus, if firms correctly acknowledge their accounting errors and irregularities (i.e., misstatements), then the frequency of restatements will be positively associated with ICMW. In contrast, if restatements are concealed, then we expect to find a less significant relationship between restatement frequency and ICMW. If U.S.-listed foreign firms' likelihood of detecting and reporting existing accounting problems varies by the home country RoL, then we expect the link between ICMW and restatement likelihood to differ by the RoL:

H2a: The relation between the probability of restatement (both errors and irregularities) and ICMW will be weaker when the U.S.-listed firm is from a country with weak RoL.

Following Hennes et al. (2008), we also distinguish between restatements that involve unintentional errors and those that likely affect intentional accounting irregularities. One possible explanation for less frequent restatements in weak RoL countries may be that in these firms, only restatements related to intentional accounting irregularities are reported. Mistakes that go unreported may be limited to minor errors. Thus, we test the above hypothesis after limiting our sample to restatements due to accounting irregularities.

As an alternative to using ICMW, which predict the probability of errors as well as accounting irregularities, we use the level of earnings management (EM) as a predictor of accounting irregularity restatements. Our null hypothesis is that there is no difference in the link between EM and accounting irregularity restatements among U.S.-listed foreign firms. If the extent of

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[12] Following Leuz et al. (2003), the alternative measure of the strength of a country's law enforcement institutions is the mean score of three law enforcement variables identified in La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998). The three measures are the original rule of law measure and two additional proxies based on assessments from risk rating agencies that attempt to capture (1) the efficiency and integrity of the country's judicial system, and (2) the degree of government corruption. Our results remain unchanged when using this alternative measure of enforcement.

compliance in detecting and reporting such restatements differs by home country RoL, then we predict the relationship between EM and restatements to increase with home country RoL:

H2b: The relation between the probability of a restatement related to accounting irregularities and earnings management will be weaker for a U.S.-listed firm from a country with weak RoL.

III. SAMPLE AND DESCRIPTIVE STATISTICS

Sample Construction

Our sample consists of all foreign firms listed on major U.S. exchanges—NYSE, NASDAQ, and AMEX—from 2000 to 2010. We include both American Depository Receipts and firms directly listed on U.S. exchanges. We exclude over-the-counter (OTC) firms because such firms are not required to register with the SEC and, therefore, do not need to follow U.S. disclosure practices (Doidge 2004).

We classify firms as foreign if they are headquartered outside the U.S., regardless of the place of incorporation, using the variable LOC from Compustat.[13] We drop firm-years that lack the financial data in Compustat and CRSP needed to compute the variables in our regression models. The selection criteria provide us with a sample of 1,357 unique foreign firms and 7,453 firm-years. The restatement sample is obtained from Audit Analytics. We define the restatement observations to reflect a single announcement rather than all the restated years. If a restatement spans multiple years, then we include the most recent year of the restating period.[14]

We partition the foreign firm sample by RoL in the home country, using the RoL index from the World Bank's Worldwide Governance Indicators, following La Porta et al. (2006).[15] We classify firms as strong and weak RoL country-firms using our sample country median (= 1.64) of the RoL index. We create a matched sample of U.S. firms to compare restatement rates of foreign and U.S. firms. The matched sample is obtained by performing an exact match on year and industry and a propensity score match based on four firm characteristics—size, leverage, ROA, and book-to-market—for each firm-year. Table 1 presents the distribution of firm-year observations and restatements for all countries from 2000 to 2010. On average, foreign firms restate less than do U.S. firms. Panel A shows that of the 2,950 firm-years in the weak RoL group, 123 (4.2 percent) were restated, while Panel B shows that of the 4,503 firm-years in the strong RoL group, 224 (5.0 percent) were later restated. For both the weak and the strong U.S. matched samples, the restatement rates are significantly higher (7.5 percent and 7.2 percent, respectively).

Table 1 also presents the frequency of different types of restatements—those due to accounting irregularities and those related to core accounts. Following prior literature, we identify restatements related to accounting irregularities via (1) *ex post* measures using external (e.g., the SEC or Department of Justice [DOJ]) or internal (board of directors) investigations (Hennes et al. 2008), and (2) *ex ante* measures using the core/non-core account classification (Palmrose et al. 2004). In

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[13] U.S. incorporated firms with foreign headquarters are often foreign firms that acquired a U.S. domiciled firm (a reverse merger) to get listed in the U.S. We include reverse-merger firms in our foreign firm sample, since such firms are better characterized as non-U.S. firms.

[14] Including only one observation for each restatement alleviates the concern that our sample may be confounded by the time it takes for a firm to correct the error (i.e., the restatement duration). We use the last year because it is closest to when the errors are detected. Our results, however, are robust to the inclusion of all the years of the restatement.

[15] The RoL variable is measured in year 2000. We follow much cross-country research (e.g., Leuz et al. 2003) that considers such fundamental characteristics as being stable over time. In Section V, we also use the adoption of international financial reporting standards (IFRS) as a positive shock to a country's reporting regime to examine a time-varying effect, unlike RoL, which is static.

TABLE 1

Sample Descriptive
2000–2010

Panel A: Distribution of Cross-Listed Firms and Restatements by Country of Domicile, Firms from Weak Rule of Law Countries

Weak Rule of Law (RoL) Countries	(1) # of Restatements Total	(2) # of Restatements Related to Core Accounts	(3) # of Restatements Due to Accounting Irregularities	(4) # of Cross-Listed Firm-Years	(5) = (1)/(4) % of Restating Firm-Years	(6) = (3)/(4) % of Restating Firm-Years, Accounting Irregularities
Argentina	2	0	0	111	2%	0%
Brazil	6	6	0	79	8%	0%
Chile	2	2	0	160	1%	0%
China	44	27	24	658	7%	4%
Colombia	0	0	0	1	0%	0%
France	17	9	1	222	8%	0%
Ghana	0	0	0	3	0%	0%
Greece	2	2	0	142	1%	0%
India	5	5	1	102	5%	1%
Indonesia	1	0	1	21	5%	5%
Israel	19	13	2	756	3%	0%
Italy	1	1	0	75	1%	0%
South Korea	3	2	1	88	3%	1%
Malaysia	0	0	0	1	0%	0%
Mexico	3	3	0	197	2%	0%
Panama	0	0	0	16	0%	0%
Peru	2	0	0	25	8%	0%
Philippines	3	2	0	16	19%	0%
Portugal	0	0	0	19	0%	0%
South Africa	5	3	1	86	6%	1%
Spain	3	3	2	69	4%	3%
Taiwan	4	2	2	80	5%	3%
Thailand	0	0	0	4	0%	0%

(continued on next page)

TABLE 1 (continued)

Weak Rule of Law (RoL) Countries	(1) # of Restatements Total	(2) # of Restatements Related to Core Accounts	(3) # of Restatements Due to Accounting Irregularities	(4) # of Cross-Listed Firm-Years	(5) = (1)/(4) % of Restating Firm-Years	(6) = (3)/(4) % of Restating Firm-Years, Accounting Irregularities
Turkey	0	0	0	10	0%	0%
Venezuela	1	1	0	9	11%	0%
Total Weak RoL Sample	123	81	35	2,950	4.2%	1.2%
Total Weak RoL U.S. Matched Sample	212	160	118	2,826	7.5%	4.2%
Total Weak RoL and U.S. Matched	335	241	153	5,776	5.8%	2.6%

Panel B: Distribution of Cross-Listed Firms and Restatements by Country of Domicile, Firms from Strong Rule of Law Countries

Strong Rule of Law (RoL) Countries	(1) # of Restatements Total	(2) # of Restatements Related to Core Accounts	(3) # of Restatements Due to Accounting Irregularities	(4) # of Cross-Listed Firm-Years	(5) = (1)/(4) % of Restating Firm-Years	(6) = (3)/(4) % of Restating Firm-Years, Accounting Irregularities
Australia	9	5	2	142	6%	1%
Austria	0	0	0	7	0%	0%
Belgium	1	1	0	17	6%	0%
Canada	87	63	14	1,706	5%	1%
Czech Republic	0	0	0	1	0%	0%
Denmark	0	0	0	25	0%	0%
Finland	3	3	0	36	8%	0%
Germany	4	3	1	169	2%	1%
Hong Kong	9	6	4	245	4%	2%
Hungary	1	1	0	15	7%	0%
Iceland	1	1	0	7	14%	0%
Ireland	7	3	2	202	3%	1%

(continued on next page)

TABLE 1 (continued)

Strong Rule of Law (RoL) Countries	(1) # of Restatements Total	(2) # of Restatements Related to Core Accounts	(3) # of Restatements Due to Accounting Irregularities	(4) # of Cross-Listed Firm-Years	(5) = (1)/(4) % of Restating Firm-Years	(6) = (3)/(4) % of Restating Firm-Years, Accounting Irregularities
Offshore Centers	29	21	14	471	6%	3%
Japan	6	6	0	245	2%	0%
Kazakhstan	2	2	0	2	100%	0%
Luxembourg	5	2	2	74	7%	3%
The Netherlands	16	12	5	258	6%	2%
New Zealand	2	2	0	18	11%	0%
Norway	0	0	0	31	0%	0%
Poland	0	0	0	2	0%	0%
Puerto Rico	0	0	0	4	0%	0%
Russia	3	3	0	41	7%	0%
Singapore	3	1	1	54	6%	2%
Sweden	1	1	0	61	2%	0%
Switzerland	14	13	8	177	8%	5%
United Kingdom	21	17	3	493	4%	1%
Total Strong RoL Sample	224	166	56	4,503	5.0%	1.2%
Total Strong RoL U.S. Matched Sample	315	225	178	4,383	7.2%	4.1%
Total Strong RoL and U.S. Matched	539	391	234	8,886	6.1%	2.6%
All Non-U.S. Firms (Strong and Weak RoL)	347	247	91	7,453	4.7%	1.2%
All Matched U.S. Firms	527	385	296	7,209	7.3%	4.1%
All Firms (Weak, Strong, and U.S. Matched)	874	632	387	14,662	6.0%	2.6%

(continued on next page)

TABLE 1 (continued)

This table shows the number of firm-year observations and the number of restatements by each home country. Weak countries are those whose rule of law index score is below the country sample median (= 1.64), while strong countries are those with an index score at or above the country median. Restatements reflect a single announcement rather than all the years restated. This is done by including only the last year of the restatement period before the announcement and dropping all prior years related to the same restatement. Restatements related to core accounts involve revenue recognition, cost of goods sold, operating expenses, or depreciation (Palmrose et al. 2004). We follow Hennes et al. (2008) and define restatements due to accounting irregularities as those that are followed by an external or board investigation. The U.S. matched sample is obtained by performing an exact match on year and industry and a propensity score match based on four firm characteristics—size, leverage, ROA, and book-to-market—for each firm-year. Offshore centers include the Bahamas, Bermuda, the Virgin Islands, The Netherlands Antilles, the Cayman Islands, the Marshall Islands, and Papua New Guinea. Since these countries are either British or Dutch territories or have a legal system that follows the British legal system, we classify them as strong countries.

Panel B, column (6), we show that foreign firms (from both weak and strong RoL countries) are less likely to report restatements due to accounting irregularities (1.2 percent) relative to their matched U.S. sample (4.1 percent). Given that prior literature suggests that earnings management behavior is more prevalent among foreign cross-listed firms than it is among U.S. firms (Lang et al. 2006), the finding that foreign firms report *fewer* restatements related to accounting irregularities is unexpected if taking detection and disclosure for granted.

Table 2, Panel A presents the descriptive statistics for the firm and governance characteristics of the foreign and matched U.S. sample. All variables are defined in Appendix A. Firms from weak RoL countries are similar in size and leverage, have higher profitability (*ROA*), and fewer growth opportunities (i.e., higher book-to-market ratio) compared to firms from the U.S. matched sample. Firms from weak RoL countries are audited by a Big 5 (or 4) audit firm less frequently (66.7 percent) relative to firms from strong RoL countries (69.0 percent) and the U.S. sample (79.14 percent); they have smaller ownership by U.S. institutions (18.1 percent versus 20.1 percent and 60.5 percent), less analyst coverage (4.07 versus 6.40 and 9.17), and are less likely to prepare financials using U.S. GAAP rather than local GAAP with reconciliation to U.S. GAAP (64.7 percent versus 81.3 percent and 99.5 percent).

Predictors of Accounting Restatements

To infer the magnitude of detection and disclosure of misstatements, we examine the sensitivity of restatements to the predictors of accounting restatements, i.e., ICMW and earnings management (EM). We use ICMW as a predictor of all restatements and EM as a predictor of only those restatements that involve accounting irregularities. Greater sensitivity suggests more prudent detection of existing accounting errors and irregularities.

Internal Control Material Weakness

ICMW are based on Section 404 disclosures obtained from Audit Analytics. Prior literature shows that good internal control systems increase the reliability of financial reporting (Doyle et al. 2007; Ashbaugh-Skaife, Collins, Kinney, and LaFond 2008). For U.S. accelerated filers, an auditor's assessment of internal controls became mandatory under Section 404 of SOX starting in November 2004. For U.S.-listed foreign firms, such an assessment was required only from fiscal years ending on or after July 2006. To ensure that our measure of ICMW is not affected by firms that voluntarily adopted the requirement earlier, we limit our analyses involving ICMW to those reported under Section 404 from 2007 to 2010, when an auditor's assessment of internal controls was mandatory.

We use the *original* ICMW reports, not the amended reports, to avoid counting ICMW that were revealed due to the restatement itself. When firms restate their financials, if their auditor had not previously reported an ICMW for the misstatement year, which is commonly the case (Rice and Weber 2012), then the auditor issues an amended ICMW report to inform investors that internal controls were not effective in the fiscal year in which the misstatement occurred. Because restatement announcements often result in subsequent ICMW disclosures, using the amended report would result in the identification of a mechanical relation between restatements and ICMW, i.e., ICMW caused by restatements, rather than the identification of ICMW disclosure as predictors of restatements. Therefore, we include only the ICMW from the original version of the ICMW effectiveness report and exclude any ICMW from the amended filing, which includes ICMW revealed by the restatement event. Using the original ICMW report allows us to focus exclusively on ICMW that precede the restatement announcement.

Panel A of Table 2 shows the frequency of ICMW in our sample. Over the 2007–2010 period, 7.9 percent of firm-years have ICMW for firms from weak RoL countries, compared with 3.4 percent for the U.S. matched sample and 5.1 percent for firms from strong RoL countries. While the

TABLE 2

Descriptive Statistics
Cross-Listed Firms and Matched U.S. Firm Sample
2000–2010

Panel A: Firm Characteristics

Variables	(1) Weak RoL Country n	(1) Weak RoL Country Mean	(2) Match U.S. Firms: Weak RoL n	(2) Match U.S. Firms: Weak RoL Mean	(3) Strong RoL Country n	(3) Strong RoL Country Mean	(4) Match U.S. Firms: Strong RoL n	(4) Match U.S. Firms: Strong RoL Mean	p-values (1) = (2)	p-values (3) = (4)
Firm Characteristics										
Size	2,950	6.601	2,826	6.621	4,503	7.187	4,383	7.272	0.748	0.102
Leverage	2,950	0.205	2,826	0.200	4,503	0.202	4,383	0.208	0.397	0.066*
ROA	2,950	0.006	2,826	−0.034	4,503	−0.015	4,383	−0.011	<0.001***	0.148
Book-to-Market	2,950	0.769	2,826	0.647	4,503	0.662	4,383	0.640	<0.001***	0.038**
Governance Variables										
ICMW (2007–2010)	866	0.079	993	0.034	1,184	0.051	1,264	0.042	<0.001***	0.303
Big Five Auditor	2,950	0.667	2,826	0.791	4,503	0.690	4,383	0.812	<0.001***	<0.001***
Analyst Coverage	2,950	4.074	2,826	9.167	4,503	6.402	4,383	11.058	<0.001***	<0.001***
Institutional Ownership	2,950	0.181	2,826	0.605	4,503	0.201	4,383	0.618	<0.001***	<0.001***
% of Firms using U.S. GAAP	2,950	0.647	2,826	0.995	4,503	0.813	4,383	0.993	<0.001***	<0.001***
% Firms with Auditors Allowing PCAOB Inspection	2,950	0.732	2,826	1.000	4,503	0.845	4,383	1.000	<0.001***	<0.001***

(continued on next page)

TABLE 2 (continued)

Panel B: Earnings Management Measures

Variables	(1) Weak RoL Country n	(1) Weak RoL Country Mean	(2) Match U.S. Firms: Weak RoL n	(2) Match U.S. Firms: Weak RoL Mean	(3) Strong RoL Country n	(3) Strong RoL Country Mean	(4) Match U.S. Firms: Strong RoL n	(4) Match U.S. Firms: Strong RoL Mean	p-values (1) = (2)	p-values (3) = (4)
EM1: *Small Positive Income*	2,950	0.058	2,826	0.066	4,503	0.059	4,383	0.079	0.063*	<0.001***
EM2: $\lvert Accruals\rvert/\lvert CFO\rvert$	2,348	1.066	2,225	0.824	3,655	0.885	3,427	0.792	<0.001***	0.001***
EM3: *Dechow/Dichev*	2,440	0.023	2,408	0.016	3,755	0.020	3,571	0.018	0.001***	0.383
EM4: $-Corr(\Delta Accrual, \Delta CFO)$	1,792	−0.173	1,986	−0.231	3,013	−0.189	3,096	−0.237	0.007***	0.007***
EM Index	2,950	3.136	2,826	2.953	4,503	2.970	4,383	2.945	<0.001***	0.410

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

This table presents the firm characteristics of the foreign cross-listed firms (by the level of home country rule of law) and their matched U.S. firms. Column (1) shows the descriptive statistics for the sample of firms from weak rule of law countries, and column (2) shows the equivalent for the matched sample of U.S. firms. Column (3) shows the descriptive statistics for firms from strong (non-U.S.) rule of law countries, and column (4) presents the equivalent for their U.S. firm matched sample. The matched U.S. sample is selected by performing a propensity score match on size, leverage, performance, and growth within the same two-digit SIC code and fiscal year. The number of observations for each variable is listed under n. The n for the *ICMW* variable is smaller because the *ICMW* variable starts from 2007, when the auditor's assessment of internal controls became mandatory for U.S.-listed foreign firms. *% of Firms using U.S. GAAP* is the percentage of firms that report using U.S. GAAP or IFRS without reconciliation (as opposed to local GAAP with reconciliation to U.S. GAAP). p-values are based on t-tests for differences in mean.

All other variables are defined in Appendix A.

univariate evidence in Table 2 makes it appear as if the extent of ICMW is higher in weak RoL countries (7.9 percent) as opposed to strong RoL countries (5.1 percent), we note that this effect is not evident in multivariate comparison once we control for various firm characteristics. In untabulated analysis, we find no significant difference between weak RoL and strong RoL in their frequencies of ICMW disclosures under Section 404.

Earnings Management

We use four earnings management measures from prior literature: (1) the proportion of small positive income (*EM1*; Burgstahler and Dichev 1997); (2) the magnitude of total accruals, measured as the ratio of the absolute value of total accruals to the absolute value of operating cash flows (*EM2*; Leuz et al. 2003); (3) accruals quality (*EM3*: Dechow and Dichev 2002); and (4) the level of discretionary smoothing (*EM4*; Francis, LaFond, Olsson, and Schipper 2005). They are all estimated at the firm-year-level using "as reported" financials, i.e., unrestated numbers. We sign the measures so that *higher* values reflect more earnings management. Detailed definitions are in Appendix A.

The underlying accounting standards used for the financials reported in Compustat vary by the firm's reporting choice. U.S.-listed foreign firms can use U.S. GAAP, IFRS (as promulgated by the International Accounting Standards Board [IASB]), or local GAAP with reconciliation to U.S. GAAP. We collect the foreign firms' accounting standards from Capital IQ. One concern with using reported financials is that the differences in accounting standards can affect the EM proxies. This can bias our inferences, particularly if the firm's reporting choice varies systematically by its home country RoL. Therefore, in addition to controlling for the accounting standard (*Reporting Standards*) in our tests, we examine the sensitivity of our results to dropping observations that report using local GAAP with reconciliation. Untabulated analysis shows that our inferences remain unchanged.

Table 2, Panel B presents the descriptive statistics of the EM measures. Firms from strong RoL countries have a higher level of earnings management relative to the matched U.S. firms on three of the four EM measures. Similarly, firms from weak RoL countries have a higher level of earnings management relative to the matched U.S. firms with the exception of the *EM1* measure. The *EM1* measure, which is the percent of firm-years with a small positive income, is higher for the matched U.S. sample than for both the weak and strong RoL samples. For our empirical analysis, we use an aggregate *EM Index* using all four measures for each firm-year.

We construct the *EM Index* by first ranking each measure and then using the average percentile rank of all four *EM* proxies. Since firm-level EM measures have measurement errors (Dechow, Ge, and Schrand 2010), we use the quintile rank of the aggregate *EM Index* in our empirical analysis.[16] Table 2, Panel B shows that firms from weak RoL countries have a significantly higher level of earnings management than the matched U.S. sample based on the overall *EM Index* (p-value $<$ 0.001). However, the difference in the *EM Index* between firms from strong RoL countries and their matched U.S. sample is not statistically significant (p-value $=$ 0.41).

Restatement Characteristics

Table 3 presents descriptive statistics relating to restatement characteristics over the 2000–2010 period. Of the 347 restatements by foreign firms, 123 (224) are by firms from weak RoL (strong

[16] Leuz et al. (2003) argue that firm-level accounting-quality metrics are less noisy when aggregated at the country level. In untabulated analysis, we compute a country-level EM measure using the country median of our sample firms. For each country-year, we rank each EM measure into percentile ranks and use the average of all four EM measures as the country-level EM index. We then examine the correlation of the country-level *EM Index* and restatement probability and find a significant positive correlation (Coeff. $=$ 0.396, p-value $<$ 0.01). The positive correlation motivates our later test using EM measures as a predictor of restatements.

TABLE 3

Restatement Characteristics of Cross-Listed Firms and Matched U.S. Firm Sample 2000–2010

	(1) Weak RoL Foreign Firms		(2) Matched U.S. Firms: Weak RoL		(3) Strong RoL Foreign Firms		(4) Matched U.S. Firms: Strong RoL		p-values	p-values
	n	Mean	n	Mean	n	Mean	n	Mean	(1) = (2)	(3) = (4)
Restatement Characteristics										
Magnitude (% of total assets)	102	−0.019	176	−0.020	166	−0.007	246	−0.006	0.938	0.891
% Material Restatements	102	0.543	176	0.629	166	0.566	246	0.621	0.184	0.211
% using Visible Restatement Disclosure	123	0.618	212	0.854	224	0.585	315	0.800	<0.001***	<0.001***
Time to Discovery	123	10.10	212	6.22	224	9.14	315	5.99	<0.001***	<0.001***
Core Account	123	0.659	212	0.755	224	0.741	315	0.714	0.059*	0.493
Consequences										
Litigation	123	0.196	212	0.163	224	0.142	315	0.167	0.466	0.470
SEC Investigation	123	0.089	212	0.061	224	0.085	315	0.124	0.338	0.151
CEO Turnover	123	0.041	212	0.118	224	0.054	315	0.111	0.017**	0.020**

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

This table presents the restatement characteristics of the foreign cross-listed firms (by the level of the home country rule of law index) and the matched U.S. firms. We use the country sample median of the rule of law index (= 1.64) to classify firms as from strong or weak rule of law countries. The rule of law index is from the Worldwide Governance Indicators created by the World Bank (Kaufmann et al. 2003) and used in La Porta et al. (2006). Column (1) shows the descriptive statistics for the sample of firms from weak rule of law countries, and specification (2) shows the equivalent for the matched sample of U.S. firms. Column (3) shows the descriptive statistics for the firms from strong rule of law countries, and column (4) presents the equivalent for their U.S. firm matched sample. The matched U.S. sample is selected by performing a propensity score match on size, leverage, performance, and growth within the same two-digit SIC code and fiscal year. The number of observations for each variable is listed under n. *% Material Restatements* is the portion of restatements that are deemed material. We follow prior literature and define *Materiality* using quantitative measures—those that have a greater than 5 percent net income impact—and qualitative measures that capture the firms' intent. We measure intent based on restatements (1) where the error changes a loss into profit, (2) that reverse an increase in earnings trend from prior years, (3) that are issued in periods of high financial distress, defined as firm-years in the highest leverage decile, or (4) that are related to the core accounts. *Core Accounts* are defined as revenue recognition, cost of goods sold, operating expenses, or depreciation. *% using Visible Restatement Disclosure* is the portion of restatements that are announced with a separate filing (e.g., form 8-K or 6-K, a press release, or non-timely filings such as NT 10-K or equivalent). See Table 1 for the list of countries with weak and strong rules of law. p-values are based on t-tests for differences in mean.
All other variables are defined in Appendix A.

RoL) countries. U.S.-listed foreign firms are less likely than the matched U.S. firms to use visible disclosure methods such as 8-K or 6-K reports. Also, the time to discovery, which is the number of months from the end of the misstatement period to the restatement announcement date, is four months longer for firms from weak RoL countries compared to U.S. matched firms. This can reflect the lack of quarterly reporting for some foreign firms. In terms of the accounts restated, foreign firms from weak RoL countries are less likely to report restatements related to core items, as described in Appendix A (Palmrose et al. 2004) relative to the matched U.S. firms.

In terms of consequences, univariate evidence in Table 3 suggests that a firm is likely to face similar regulatory or legal actions regardless of its country of origin once it reports a restatement. Securities litigation is triggered by 19.6 percent (16.3 percent) of restatements by firms from weak RoL countries (the matched U.S. sample). The SEC investigates 8.9 percent (6.1 percent) of firms from weak RoL countries (the matched U.S. sample). Both of these differences are not statistically significant. As a comparison, 14.2 percent and 8.5 percent of restatements by firms from strong RoL countries lead to securities litigation and an SEC investigation respectively, figures that are lower than, but not statistically different from, those observed in the matched sample of U.S. firms.

We observe in Table 3 significant differences in CEO turnover following restatements: CEO turnover rates for firms from both weak and strong RoL countries (4.1 percent and 5.4 percent, respectively) are significantly lower than in the matched samples of U.S. firms (11.8 percent and 11.1 percent, respectively). This indicates that CEO turnover, which is another method for identifying restatements related to severe accounting irregularities for U.S. firms (Badertscher, Phillips, Pincus, and Rego 2009), may have limited power in an international context. For this reason, we rely on other measures, using (1) external or internal investigations, and (2) the core/non-core account classification.

IV. EMPIRICAL RESULTS

Frequency of Restatements and the Home Country Effect

We first examine how the likelihood of restating differs for U.S.-listed foreign firms and matched U.S. firms. Since we use *ICMW* as the predictor, the sample period for this analysis starts from 2007 when auditor's assessment of ICMW became mandatory for U.S.-listed foreign firms, and hence the sample size is smaller than in Table 1 (the sample size for this analysis is provided in the description of the *ICMW* variable in Table 2, Panel A). We use the following logit model:

$$\begin{aligned} Restatement_{i,t} = {} & \beta_0 + \beta_1 \times Foreign\ Firm\ Indicator_i + \beta_2 \times ICMW_{i,t,prior} \\ & + \beta_3 \times EM\ Index_{i,t,prior} + \beta_{4\text{-}14} \times Controls_{i,t-1} + Industry\ FE + Year\ FE \\ & + \varepsilon_{i,t}. \end{aligned} \tag{1}$$

The dependent variable, $Restatement_{i,t}$, equals 1 if firm i restated financial statements for year t, and 0 otherwise. For restatements that affect multiple firm-years, we include only the most recent year of the restatement, as described earlier. *Foreign Firm Indicator* is the primary variable of interest and equals 1 for firms from a non-U.S. country, and 0 otherwise.

We control for the likelihood of accounting errors and irregularities using a measure of the quality of internal control systems (*ICMW*) and the earnings management measure (*EM Index*). $ICMW_{i,t,prior}$ is an indicator variable equal to 1 if the firm reported an ICMW for year t, *prior to* identifying the need to restate the financials, and 0 otherwise. The subscript *prior* denotes that the measure represents values before identifying the need to restate year t's financials. As discussed

earlier, we use the original ICMW effectiveness report to focus exclusively on ICMW that precede the discovery of year t's restatement. $EM\ Index_{i,t,prior}$ is the earnings management index variable constructed using the unrestated financials. For restatements that affect multiple firm-years, we use the EM measure for the most recent year of the restatement. We also use a number of control variables hypothesized to affect the likelihood of a restatement. Firm characteristics include size, leverage, profitability, and growth (DeFond and Jiambalvo 1991; Badertscher et al. 2009). We also include complexity, measured as the number of business segments, and measures of the firm's monitoring environment: auditor, analyst following, and institutional ownership. Following Lang et al. (2006), we control for whether the foreign firm reports using U.S. GAAP (or IFRS) or using local GAAP with reconciliation to U.S. GAAP. Finally, we include year and industry fixed effects to control for unobservable time and industry factors that may affect the restatement probability. Standard errors are clustered by firm.

Next, we repeat our analysis within the foreign sample and examine whether the probability of restatements varies by home country RoL (H1) using the following logit model:

$$\begin{aligned} Restatement_{i,t} = {} & \beta_0 + \beta_1 \times Weak\ RoL\ Indicator_i + \beta_2 \times ICMW_{i,t,prior} \\ & + \beta_{4\text{-}14} \times Firm\ Controls_{i,t-1} + \beta_{15\text{-}17} \times Country\ Controls_{c,t-1} \\ & + Industry\ FE + Year\ FE + \varepsilon_{i,t}. \end{aligned} \tag{2}$$

Weak RoL Indicator is an indicator variable that equals 1 for firms from weak RoL countries, and 0 otherwise. Additionally, to mitigate the possibility that the weak RoL partition is capturing other control characteristics, we include country-level controls, such as differences in local accounting versus U.S. GAAP (*Accounting Difference*), capital market development (*Country Market Cap*), economic growth (*Country GDP Growth*), or differences in auditor legal liability (*Auditor Liability*), all of which may be associated with the propensity to restate.

Table 4 presents the results from estimating the models in Equations (1) and (2). First, we include all sample restatements (columns (1) and (2)) and then only restatements related to accounting irregularities (columns (3) and (4)). Column (1) presents results from Equation (1) using the foreign and matched U.S. samples. The coefficient on *Foreign Firm Indicator* is negative and statistically significant (Coeff. = −0.637, p-value = 0.007), suggesting that U.S.-listed foreign firms are less likely to restate their financials relative to matched U.S. firms. The estimated coefficient suggests that the probability of foreign firms restating is 1.9 percent when evaluated at the mean of the control variables. The comparable probability of the U.S. matched sample is 3.5 percent,[17] suggesting that foreign firms are 46 percent less likely to restate their financials than the U.S. matched firms, after controlling for other determinants of restatements.

Column (2) of Table 4 shows the estimated coefficients of Equation (2) using only the foreign firm sample. The coefficient on the *Weak RoL Indicator* is negative and statistically significant (Coeff. = −0.795, p-value = 0.071), indicating that firms from weak RoL countries are less likely to restate their financials compared to firms from strong RoL countries. In economic terms, this implies that firms from weak RoL countries are 42 percent less likely to restate their financials than firms from strong RoL countries, after controlling for other determinants of restatements.[18]

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[17] To compute the predicted probabilities, we evaluate each coefficient at the sample mean and calculate the predicted probability as 1/(1 + exponent of the negative summed value). For example, for the matched U.S. sample, the summed value of each coefficient at the sample mean is −3.30. The resulting calculation of the predicted probability is 1/(1 + exp(3.30)) = 3.5 percent. Similarly, the calculation for the foreign firm sample is 1/(1 + exp(3.95)) = 1.9 percent.

[18] The estimates suggest that the probability of firms from weak RoL countries restating is 1.10 percent when evaluated at the mean of the control variables. For foreign firms from strong RoL countries, the comparable probability is 1.88 percent.

TABLE 4

Restatement Probability of Foreign Firms Listed in the U.S.
2007–2010

$$Restatement_{i,t} = \beta_0 + \beta_1 \times Foreign\ Firm(Weak\ RoL)\ Indicator_{i,t} + \beta_2 \times ICMW_{i,t,prior} + \beta_3 \times EM\ Index_{i,t,prior} + \beta_{4\text{-}14} \times Controls_{i,t-1} + Industry\ FE + Year\ FE + \varepsilon_{i,t}.$$

	All Restatements		Irregularities Only	
Variable (Predicted Sign)	**(1) All Foreign and U.S. Matched**	**(2) All Foreign Firms**	**(3) All Foreign and U.S. Matched**	**(4) All Foreign Firms**
Foreign Firm Indicator (–)	**–0.637******		**–1.026******	
	[0.007]		**[0.005]**	
Weak RoL Indicator (–)		**–0.795***		**–1.646****
		[0.071]		**[0.018]**
ICMW (+)	1.881***	1.161***	2.520***	1.376**
	[0.000]	[0.003]	[<0.001]	[0.015]
EM Index (+)	0.135*	0.064	0.117	–0.139
	[0.071]	[0.570]	[0.267]	[0.346]
Firm Controls				
Size	–0.153*	–0.110	–0.276**	–0.225
	[0.053]	[0.278]	[0.030]	[0.213]
Leverage	0.430	0.396	0.431	–0.867
	[0.440]	[0.609]	[0.554]	[0.530]
ROA_Current	–0.665	–0.364	–1.199	–1.179
	[0.393]	[0.812]	[0.213]	[0.468]
ROA_Lagged	1.162	0.101	2.815*	1.694
	[0.243]	[0.955]	[0.051]	[0.472]
Book-to-Market	0.118	0.228	0.145	0.428
	[0.496]	[0.402]	[0.534]	[0.323]
Big Five Auditor	–0.263	–0.910***	–0.329	–1.019*
	[0.286]	[0.004]	[0.362]	[0.076]
Analyst Coverage	–0.000	0.016	–0.000	–0.024
	[0.977]	[0.511]	[0.985]	[0.557]
Institutional Ownership	–0.003	–0.248	0.216	–0.061
	[0.991]	[0.587]	[0.504]	[0.919]
Sales Growth	–0.408	–0.012	–0.234	0.375
	[0.175]	[0.978]	[0.544]	[0.560]
Segments	–0.268*	0.064	–0.326	0.443
	[0.095]	[0.811]	[0.177]	[0.304]
Reporting Standard	1.881***	1.330		
	[<0.001]	[0.230]		
Country Controls				
Accounting Difference		0.333		0.555
		[0.688]		[0.709]
Country Market Cap		0.000		–0.000
		[0.795]		[0.256]
Country GDP Growth		9.610*		38.080***
		[0.081]		[0.000]

(continued on next page)

TABLE 4 (continued)

Variable (Predicted Sign)	All Restatements		Irregularities Only	
	(1) All Foreign and U.S. Matched	(2) All Foreign Firms	(3) All Foreign and U.S. Matched	(4) All Foreign Firms
Auditor Liability		0.702 [0.338]		1.331 [0.316]
Pseudo R^2	0.111	0.108	0.198	0.212
# Obs.	4,307	1,986	4,147	1,617
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.
This table reports the estimation from a logistic regression of Models (1) and (2). In columns (1) and (2), the dependent variable $Restatement_{i,t}$ equals 1 if firm i restated financial statements for year t, and 0 otherwise. In columns (3) and (4), the dependent variable is an indicator variable that takes a value of 1 if firm reported an irregularities restatement for year t, and 0 otherwise. We follow Hennes et al. (2008) and define restatements due to accounting irregularities as those that are followed by an external or board investigation. Estimates of the home country effect are presented in bold. *Foreign Firm Indicator* is an indicator variable equal to 1 for non-U.S. firms, and 0 otherwise. *Weak RoL* is an indicator variable equal to 1 for firms from countries with a weak rule of law, and 0 otherwise. We use the country sample median of the rule of law index (= 1.64) to classify firms as from strong or weak rule of law countries. The rule of law index is from the Worldwide Governance Indicators created by the World Bank (Kaufmann et al. 2003) and used in La Porta et al. (2006). $ICMW_{i,t,prior}$ is an indicator variable equal to 1 if the firm reported an ICMW for year *t*, *prior to* identifying the need to restate the financials, and 0 otherwise. We use the original IC effectiveness report (rather than the amended reports) to focus exclusively on ICMW that precede the announcement of the restatement for year *t*. $EM\ Index_{i,t,prior}$ is the earnings management index variable constructed using the unrestated financials as described in Section III. Standard errors are clustered at the firm level.
All other variables are defined in Appendix A.

In column (3) of Table 4, we examine the effect of the *Foreign Firm Indicator* on irregularities restatements. Following Hennes et al. (2008), we define irregularities restatements as restatements that involve an external or internal investigation.[19] The coefficient on *Foreign Firm Indicator* is negative and statistically significant (Coeff. = −1.026, p-value = 0.005) and the economic magnitude is greater than the earlier analysis using all restatements. Column (4) estimates the weak RoL effect using only the foreign firm sample. We find that the *Weak RoL Indicator* is negative and statistically significant (Coeff. = −1.646, p-value = 0.018), with greater economic magnitude compared to column (2). Overall, the results imply that foreign firms, especially those from weak RoL countries, are less likely to restate than comparable U.S. firms. The lower restatement rate of foreign (weak RoL) firms becomes even more pronounced for restatements related to accounting irregularities. We observe a statistically insignificant coefficient on the *EM Index* in columns (2), (3), and (4). As we will see later (in Tables 4 and 5), earnings management is a good predictor of restatements only in the U.S. and strong RoL countries. The coefficient loses its significance in the combined samples, likely due to the inclusion of the weak RoL sample.

[19] Audit Analytics identifies restatements that are accompanied by a board of directors' investigation. These do not include investigations initiated by managers, as in Hennes et al. (2008).

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TABLE 5

Home Country Rule of Law and Restatement Probability Conditional on Internal Control Material Weaknesses (ICMW) 2007–2010

Panel A: Univariate Analysis: Likelihood of Restatements Conditional on ICMW, by Home Country Rule of Law

	All Foreign Firms (n = 2,088)	U.S. Matched Firms (n = 2,320)	Foreign: Weak RoL (n = 876)	U.S. Matched: Weak RoL (n = 1,025)	Foreign: Strong RoL (n = 1,212)	U.S. Matched: Strong RoL (n = 1,295)
ICMW	10.94%	32.18%	7.35%	23.53%	15.00%	37.74%
No ICMW	2.29%	3.55%	2.13%	4.28%	2.40%	2.97%
Differences	8.65%	28.64%	5.22%	19.25%	12.60%	34.76%
[p-value]	[<0.001]	[<0.001]	[0.009]	[<0.001]	[<0.001]	[<0.001]

Panel B: Likelihood of Restatements Conditional on ICMW, by Home Country Rule of Law

$$\text{Model: } Restatement_{i,t} = \beta_0 + \beta_1 \times ICMW_{i,t,prior} + \beta_2 \times EM\ Index_{i,t,prior} + \beta_{3\text{-}14} Firm\ Controls_{i,t-1} + \beta_{15\text{-}17} Country\ Controls_{c,t-1} + Industry\ FE + Year\ FE + \varepsilon_{i,t}. \quad (3)$$

Variable (Predicted Sign)	(1) All Foreign Firms	(1) U.S. Matched Firms	(2) Foreign Firms: Weak RoL	(2) U.S. Matched Firms: Weak RoL	(3) Foreign Firms: Strong RoL	(3) U.S. Matched Firms: Strong RoL
ICMW (+)	**1.191*****	**2.507*****	**0.587**	**2.083*****	**2.038*****	**3.160*****
	[0.003]	**[<0.001]**	**[0.259]**	**[<0.001]**	**[<0.001]**	**[<0.001]**
F-test						
F-stats	**6.59**		**4.10**		**3.01**	
[p-value]	**[0.010]****		**[0.043]****		**[0.083]***	
EM Index (+)	0.049	0.204*	−0.367*	0.261*	0.310**	0.120
	[0.668]	[0.059]	[0.065]	[0.061]	[0.034]	[0.438]
Firm Controls						
Size	−0.079	−0.118	−0.486**	−0.148	0.192*	−0.078
	[0.437]	[0.373]	[0.034]	[0.402]	[0.098]	[0.694]
Leverage	0.355	−0.029	0.908	0.074	0.094	0.165
	[0.647]	[0.971]	[0.517]	[0.951]	[0.931]	[0.879]
ROA_Current	−0.280	−0.769	−0.795	0.648	−0.909	−1.565
	[0.852]	[0.418]	[0.698]	[0.641]	[0.613]	[0.235]
ROA_Lagged	−0.111	2.069*	1.540	1.303	−2.121	3.347**
	[0.950]	[0.086]	[0.632]	[0.438]	[0.294]	[0.040]
Book-to-Market	0.203	0.029	0.135	−0.013	0.324	0.038
	[0.455]	[0.905]	[0.753]	[0.972]	[0.384]	[0.899]
Big Five Auditor	−0.931***	0.088	−1.424**	0.361	−0.392	−0.364
	[0.004]	[0.798]	[0.021]	[0.458]	[0.379]	[0.451]

(continued on next page)

TABLE 5 (continued)

Variable (Predicted Sign)	(1) All Foreign Firms	(1) U.S. Matched Firms	(2) Foreign Firms: Weak RoL	(2) U.S. Matched Firms: Weak RoL	(3) Foreign Firms: Strong RoL	(3) U.S. Matched Firms: Strong RoL
Analyst Coverage	0.020	−0.017	0.037	−0.013	−0.005	−0.021
	[0.410]	[0.466]	[0.461]	[0.662]	[0.857]	[0.582]
Institutional Ownership	−0.264	0.054	−1.191	−0.109	0.006	0.269
	[0.569]	[0.859]	[0.130]	[0.807]	[0.992]	[0.549]
Sales Growth	−0.058	−0.851*	0.073	−1.277	−0.441	−0.593
	[0.894]	[0.062]	[0.921]	[0.107]	[0.473]	[0.290]
Segments	0.048	−0.610***	0.657	−1.010***	−0.367	−0.352
	[0.858]	[0.003]	[0.160]	[0.001]	[0.177]	[0.246]
Reporting Standard	1.557	−1.573	0.498			−1.755*
	[0.160]	[0.150]	[0.663]			[0.079]
Country Controls						
Accounting Difference	−0.004		2.849**		−0.748	
	[0.997]		[0.044]		[0.601]	
Country Market Cap	0.000	−0.000	0.000	0.000	−0.000	−0.000
	[0.798]	[0.583]	[0.839]	[0.872]	[0.105]	[0.320]
Country GDP Growth	4.847	−3.430	7.538	−7.932	−4.820	−0.745
	[0.314]	[0.576]	[0.455]	[0.319]	[0.642]	[0.938]
Auditor Liability	1.078		0.679		0.647	
	[0.147]		[0.715]		[0.459]	
Pseudo R^2	0.103	0.158	0.215	0.153	0.152	0.222
# Obs.	1,986	2,244	774	962	1,136	1,254
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.
Panel A shows the percentage of firm-years in each sample group that subsequently reports a restatement, split into firm-years with reported internal control deficiencies during the same year, prior to identifying the need to restate the financials. The sample period starts from 2007, when the auditor's assessment of internal controls became mandatory for U.S.-listed foreign firms. Panel B reports the estimation from a logistic regression. The dependent variable is an indicator variable that equals 1 if firm *i* restated financial statements for year *t*, and 0 otherwise. We use the country sample median of the rule of law index (= 1.64) to classify firms as from strong or weak rule of law countries. The rule of law index is from the Worldwide Governance Indicators created by the World Bank (Kaufmann et al. 2003) and used in La Porta et al. (2006). Coefficient estimates and p-values [in brackets] are from seemingly unrelated regressions of restatement probability on ICMW and other controls. F-tests compare the coefficients of the *ICMW* variable for the weak rule of law country sample and their U.S. matched sample (Model (2)), as well as the strong rule of law country sample and its matched sample (Model (3)). Estimates of the coefficients included in the F-tests are shown in bold. Standard errors are clustered at the firm level. p-values are based on t-tests for differences in mean.
All other variables are defined in Appendix A.

Home Country Effect and Restatements Conditional on the Level of ICMW

The evidence discussed in the previous section suggests that foreign firms, especially from weaker RoL countries, report fewer restatements. There are two ways one can interpret the lower restatement rates. First, firms from weak RoL countries can have fewer accounting errors and irregularities and, therefore, less need to restate their financials. The other interpretation is that

errors and irregularities are more likely to go undisclosed in firms from weak RoL countries. So far, we have controlled for the level of existing errors and irregularities using proxies of ICMW and EM and drawn inferences consistent with the second interpretation. In other words, we inferred less detection and reporting from the lower restatement rates by conditioning on ICMW and EM.

ICMW and earnings management are our proxies for the underlying extent of weaknesses in the accounting system. We conjecture that the lower rate of restatements seen in Table 4 is because in U.S. companies, underlying weaknesses are more likely to result in a restatement than in foreign companies. Similarly, in countries with better enforcement, underlying weaknesses are more likely to result in restatements than in countries with weaker enforcement. Therefore, we examine whether ICMW are more strongly related to restatements of U.S. firms compared to foreign firms. We repeat this comparison for companies in weak and strong RoL countries. Empirically, we test for differences in the extent of the detection and disclosure of existing irregularities using the *sensitivity* of the restatement rates to the level of ICMW, i.e., we compare the coefficient on *ICMW* across the foreign versus U.S. samples (and weak versus strong RoL samples).

We use the logit model in Equation (3) and compare the difference in the coefficients on the *ICMW* variable for the foreign and U.S. matched sample using a seemingly unrelated regression (SUR) model. The advantage of separately estimating the coefficients across the different samples is that it allows all predictors, including *ICMW,* to vary across the two samples. The specification is equivalent to interacting all the control variables with the foreign (or RoL) indicator variable to capture the differences in the prediction model for the two samples:

$$\begin{aligned} Restatement_{i,t} = {} & \beta_0 + \beta_1 \times ICMW_{i,t,prior} + \beta_2 \times EM\ Index_{i,t,prior} + \beta_{3\text{-}14} Firm\ Controls_{i,t-1} \\ & + \beta_{15\text{-}17} Country\ Controls_{c,t-1} + Industry\ FE + Year\ FE + \varepsilon_{i,t}. \end{aligned} \tag{3}$$

We compare the sensitivity of restatements to $ICMW_{i,t,prior}$ across the foreign firms and the matched U.S. samples and expect the association to be weaker for foreign firms (i.e., $\beta_{1,Foreign\ Firm} < \beta_{1,Matched\ U.S.\ Firm}$).[20]

Panel A of Table 5 presents univariate evidence on the restatement probability using the *ICMW* indicator to identify firms with and without ICMW. In each sample, firms with a ICMW are always more likely to restate than those that report no ICMW. More importantly, the differences in restatement probability between firms with and without ICMW are strongly related to home country enforcement. The weak RoL sample (matched U.S. sample) shows a 5.22 percent (19.25 percent) difference in the restatement probability for firms with and without ICMW. This difference increases to 12.60 percent (34.76 percent) for the strong RoL group (corresponding U.S. matched sample).

Table 5, Panel B presents the results of estimating Equation (3). Column (1) shows the estimated coefficients for all U.S.-listed foreign firms and the U.S. matched sample. For both samples, ICMW are related to restatements, but the relation is stronger for the U.S. sample (F-test, p-value = 0.01), suggesting that foreign firms' restatements are less sensitive to ICMW than those

[20] It is possible that ICMW reporting is also subject to the strength of detection and reporting incentives, i.e., the two-step reporting process. If factors that drive the reluctance to report ICMW are correlated with home country RoL, then our findings may be subject to measurement error. Gong et al. (2013) show that the effect of home country enforcement on ICMW disclosure was significantly reduced after SOX 404, in effect since 2007, when Section 404 IC assessment by the auditor became mandated for all accelerated filers. In untabulated analysis, we find no post-2007 effect of RoL on ICMW reporting. Thus, we restrict our sample period to after 2007. In additional analyses, we show that our findings are not driven by potential measurement error in the *ICMW* variable (see Section V). However, we cannot fully discount the possibility that biases in ICMW reporting affect our results.

of comparable U.S. firms. We interpret the lower sensitivity as evidence of less stringent detection/disclosure of accounting misstatements for the foreign sample relative to U.S. firms.[21]

Column (2) of Table 5, Panel B shows the estimated coefficients for U.S.-listed firms from weak RoL countries and their matched sample. For weak RoL countries, there is no significant relation between *ICMW* and restatement probability (Coeff. = 0.587, p-value = 0.259), while the relation is positive and significant (Coeff. = 2.038, p-value < 0.001) for the U.S. matched sample. In economic terms, having ICMW increases the probability of restatement from 1 percent to 3.7 percent for firms from weak RoL countries, compared to an increase from 2.7 percent to 21.3 percent for comparable U.S. firms.[22] The F-test shows that the difference in the coefficients is statistically significant (p-value = 0.043). Column (3) results show that *ICMW* is a strong predictor of restatements for both the strong RoL sample and its matched U.S. sample. The F-test shows that the relationship is stronger for U.S. companies than it is for companies from strong RoL countries. The results imply that firms' likelihood of detecting and reporting existing accounting problems increases with the home country RoL.[23]

The Home Country Effect and Restatements Related to Accounting Irregularities

One possible explanation for the lower restatement rate in weak RoL country-firms is that these firms avoid restating minor errors and report only accounting irregularities. While results in Table 4 suggest that weak RoL country-firms report fewer restatements related to accounting irregularities, we directly test for the difference in detection using the following model:

$$\begin{aligned} \textit{Restatement Irregularities}_{i,t} = {} & \beta_0 + \beta_1 \times \textit{EM Index}_{i,t,prior} + \beta_{2\text{-}13}\textit{Firm Controls}_{i,t-1} \\ & + \beta_{14\text{-}16}\textit{Country Controls}_{c,t-1} + \textit{Industry FE} + \textit{Year FE} + \varepsilon_{i,t}. \end{aligned} \tag{4}$$

There are two differences between Equation (4) and Equation (3). First, we limit the sample to restatements related to accounting irregularities. The dependent variable, $\textit{Restatement Irregularities}_{i,t}$, equals 1 if the financials for firm *i* in year *t* were restated due to an accounting irregularity, and 0 otherwise. We define restatements related to accounting irregularities in two ways: restatements involving an external (SEC, DOJ) or internal (board of directors) investigation; and next, based on *ex ante* classification, as those involving core versus non-core items (Palmrose et al. 2004). Second, we use the *EM Index* instead of *ICMW* as the predictor. The *EM Index* is likely to predict accounting irregularities because earnings management implies that there was an intent to commit accounting mistakes (Dechow, Ge, Larson, and Sloan 2011). Because we omit the *ICMW* variable, the sample for this test starts from 2000 instead of 2007.

We estimate Equation (4) as a SUR model to compare estimates on our variable of interest, the β_1 coefficients, across the foreign firms and their matched U.S. sample. To the extent that earnings management in firms from weak RoL countries is less likely to be revealed through restatements, we expect the β_1 coefficient to be less positive for such firms relative to U.S. firms.

Table 6, Panel A presents the results of the logistic regression of Equation (4), where *Restatement Irregularities* is defined as restatements involving an internal or external investigation.

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[21] Note that the reporting discretion of non-U.S. firms is gauged relative to U.S. firms. Since we cannot observe the true underreporting rate of any group of firms, we cannot make inferences about the absolute levels of underreporting.

[22] To compute the predicted probabilities, we evaluate each coefficient at the mean of the respective samples and calculate the predicted probability as 1/(1 + exponent of the negative summed value).

[23] We also compare the *Restatement-ICMW* sensitivity for the weak and strong RoL samples (i.e., within the foreign sample). Unreported F-test shows that the *ICMW* coefficient in the strong RoL sample is significantly higher than in the weak RoL sample when the two samples are compared with each other (p-value < 0.039).

TABLE 6

Home Country Rule of Law and Restatements due to Accounting Irregularities 2000–2010

Model: $$Restatement\ Irregularities_{i,t} = \beta_0 + \beta_1 \times EM\ Index_{i,t,prior} + \beta_{2\text{-}13} Firm\ Controls_{i,t-1} + \beta_{14\text{-}16} Country\ Controls_{c,t-1} + Industry\ FE + Year\ FE + \varepsilon_{i,t}. \quad (4)$$

Panel A: Likelihood of Restatement Related to Accounting Irregularities (Hennes et al. 2008), Conditional on EM

	(1)		(2)		(3)	
Variable (Predicted Sign)	**All Foreign Firms**	**U.S. Matched Firms**	**Foreign Firms: Weak RoL**	**U.S. Matched: Weak RoL**	**Foreign Firms: Strong RoL**	**U.S. Matched: Strong RoL**
EM Index (+)	0.083	0.169***	−0.084	0.219***	0.206*	0.137**
	[0.326]	[0.002]	[0.532]	[0.006]	[0.058]	[0.044]
F-test						
F-stats	**0.74**		**3.77**		**0.29**	
p-value	**[0.391]**		**[0.052]***		**[0.591]**	
# Obs.	6,511	6,978	1,929	2,717	3,511	4,246
Pseudo R^2	0.106	0.095	0.237	0.136	0.092	0.089
Firm controls in Table 5			Yes	Yes	Yes	Yes
Country controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Likelihood of Core Account Restatement (Palmrose et al. 2004), Conditional on EM

	(1)		(2)		(3)	
Variable (Predicted Sign)	**All Foreign Firms**	**U.S. Matched Firms**	**Foreign Firms: Weak RoL**	**U.S. Matched: Weak RoL**	**Foreign Firms: Strong RoL**	**U.S. Matched: Strong RoL**
EM Index (+)	0.030	0.148***	−0.101	0.203***	0.098	0.105*
	[0.576]	[0.001]	[0.284]	[0.004]	[0.138]	[0.058]
F-test						
F-stats	**2.70**		**6.67**		**0.01**	
p-value	**[0.100]***		**[0.010]*****		**[0.933]**	
# Obs.	7,353	7,067	2,885	2,736	4,194	4,293
Pseudo R^2	0.069	0.062	0.138	0.084	0.074	0.067
Firm controls in Table 5			Yes	Yes	Yes	Yes
Country controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

(continued on next page)

TABLE 6 (continued)

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test. This table reports the estimation from a logistic regression. The dependent variable *Restatement Irregularities*$_{i,t}$ is an indicator variable that takes a value of 1 if firm reported an irregularities restatement for year *t*, and 0 otherwise. In Panel A, we define restatements due to accounting irregularities as those that are subsequently followed by external board investigations (Hennes et al. 2008). Panel B defines restatements due to accounting irregularities as those that involved a core account (Palmrose et al. 2004). Core accounts are defined as revenue recognition, cost of goods sold, operating expenses, or depreciation. *EM Index* is the average percentile rank for each firm for the year across the four (or as many as are available) measures of earnings management. We use the unrestated numbers to construct the underlying EM measures. Higher values indicate higher earnings management. Coefficient estimates and p-values [in brackets] are from seemingly unrelated regressions of restatement probability on the *EM Index* and other controls. F-tests compare the coefficients of the aggregate *EM Index* variable for the weak rule of law country sample and its U.S. matched sample, as well as the strong rule of law country sample and its matched sample. Estimates of the coefficients included in the F-tests are shown in bold. Standard errors are clustered at the firm level. p-values are based on t-tests for differences in mean. All other variables are defined in Appendix A.

Panel B shows our results using the *ex ante* definition, i.e., restatements involving core items, following Palmrose et al. (2004).

In Panel A, column (1) of Table 6, we estimate the model using the foreign firm and the U.S. matched samples. The estimated coefficient on the *EM Index* shows that there is a significant relation between earnings management and restatement probability in the U.S. matched firm sample, but not in the foreign sample. However, the F-test shows that the difference between the two coefficients is not statistically significant. Next, we divide the foreign firms into those from strong versus weak RoL countries and compare the *EM Index* coefficient to the estimates from their respective matched U.S. samples. Column (2) estimates a SUR model using the weak RoL firms and their U.S. matched sample. The *EM Index* coefficient is positive and significant only for the U.S. matched sample (= 0.219, p-value = 0.006), but not for the foreign firm sample. F-tests show that the difference in the coefficients is statistically significant (p-value = 0.052). Column (3) shows results using the strong RoL country sample and its matched U.S. sample. The coefficient on *EM Index* is positive and significant for both the strong RoL and U.S. matched sample; the difference in coefficients is not statistically significant (p-value = 0.591). We find generally similar results in Panel B using core items to classify accounting irregularities.

The results suggest that, relative to similar U.S. firms, restatements by firms from weak RoL countries are not as reflective of underlying accounting properties as they are for firms from strong RoL countries.[24] The tendency of high-EM firms to report irregularity type restatements is weakest for foreign firms with weak enforcement. This suggests that avoidance of restatements is true even for restatements that likely reflect irregularities, as opposed to minor errors.

Sensitivity Analysis

Measurement Error in the Reporting of Internal Control Weaknesses

A potential concern with our tests is the measurement error in the *ICMW* variable as a proxy for the likelihood of accounting misstatements. Firms have discretion in both ICMW reporting and restatement reporting. Thus, factors that drive ICMW reporting can also affect restatement

[24] In untabulated analysis, we compare the two foreign samples of strong and weak rule of law firms to each other, without the matched U.S. firm sample. An F-test shows that the coefficient on the EM variable in the strong RoL sample is significantly higher than in the weak RoL sample (p-value = 0.0084).

outcomes. While we include various control variables to account for this effect, it is possible that our empirical model suffers from a misspecification, especially if the measurement error in ICMW systematically varies with RoL. To mitigate this concern, we take the following three approaches.

First, we test whether ICMW varies across strong versus weak RoL countries by regressing *ICMW* on *RoL* and other control variables (as in Table 4). In untabulated results, we find an insignificant coefficient on *RoL,* suggesting that ICMW reporting is not systematically different across weak and strong RoL countries. This alleviates the concern that both restatements and ICMW vary by RoL. Second, we clarify that what we infer from the association between restatements and ICMW is the *incremental* discretion in restatement reporting. Once a firm reports an ICMW, observing a lower restatement rate represents greater discretion in restatement reporting. In other words, our null hypothesis is that, conditional on firms that report ICMW, the likelihood of restatements is similar across countries in the absence of discretion in restatement reporting. Therefore, an assumption underlying our empirical tests is that the relative discretion in the reporting of restatements is incrementally greater than the discretion in ICMW reporting.[25]

Third, we run additional tests using an alternative measure of ICMW that is not subject to a firm's reporting discretion—*predicted* ICMW. We estimate predicted *ICMW* using the prediction model from prior literature (Ashbaugh-Skaife et al. 2008; Doyle et al. 2007; Rice and Weber 2012). Specifically, we include in the model firm size, firm age, presence of losses, extreme sales growth, mergers and acquisitions (M&A) indicator, restructuring changes, acquisition activities in the prior year, foreign currency adjustments, CEO or CFO turnover in the previous year, stock/debt issuance in the prior year, ICMW in the prior year, restatement in the prior year, audit fees, nonaudit fees, Big 5 auditor, and auditor resignation. The estimated coefficients (untabulated) show that larger firms and firm-years with losses, M&A, and auditor resignation are more likely to report ICMW. We calculate the fitted probabilities and estimate the predicted probabilities as an indicator variable.[26]

We repeat our analysis from Table 5 (untabulated) using the predicted *ICMW* variable. We find that the relation between predicted ICMW and restatement probability is positive and significant for U.S. firms (Coeff. = 0.974, p-value = 0.056), while the relation is *negative* (Coeff. = −1.430, p-value = 0.018) for firms from weak RoL countries. The F-test shows that the difference in the weak RoL firms and the U.S. sample is significant. For the strong RoL sample and its matched U.S. sample, predicted *ICMW* is a strong predictor of restatements, but with no significant difference between the two samples. This suggests that our findings are robust to using an alternative measure of ICMW.

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[25] Prior literature provides potential reasons why discretion in restatement reporting is greater than discretion in ICMW reporting. Studies show that while ICMW have information content and are useful disclosures, restatements have a significantly greater negative impact on the firm than do ICMW. Such negative consequences of restatements can provide greater incentives to managers and auditors to exercise discretion in restatement reporting. For example, Beneish, Billings, and Hodder (2008, 665) show that Section 404 ICMW disclosures have "no noticeable impact on stock prices or firms' cost of capital." Similarly, Ogneva, Raghunandan, and Subramanyam (2007) show that internal control weakness reports are not directly associated with higher cost of equity. Ashbaugh-Skaife et al. (2009) do show that IC weaknesses are associated with higher cost of equity, but their sample includes both SOX 302 and 404 cases. These findings are in stark contrast to the restatement studies that show a strong negative market reaction and higher cost of equity effect following restatements (Palmrose et al. 2004).

[26] For the indicator variable, we cut the fitted probability at the point where the sensitivity and the inverse of the specificity of the ROC (receiver operating characteristic) curve are jointly maximized. The numerical value of this cut-off point is 0.05. We use the country sample median of the rule of law index (= 1.64) to classify firms as from strong or weak rule of law countries.

Restatement Frequency: Excluding Quarterly Restatements

One possible reason for fewer restatements by foreign firms is the difference in quarterly reporting rules. Foreign registrants do not have a dedicated form like the 10-Q for quarterly financials (they use form 6-K) and do not have to report quarterly if they do not do so in the home country. We examine the robustness of Table 5′s results after excluding quarterly restatements. Results (untabulated) lead to similar inferences as in Table 5. In all three columns (U.S. versus foreign, weak RoL versus U.S. matched, and strong RoL versus U.S. matched), the ICMW coefficient is positive and significantly higher for the U.S. sample than the corresponding foreign sample. The weak RoL sample does not show a significant coefficient on *ICMW*, indicating that ICMW is not a predictor of restatements in weak RoL countries, unlike in the U.S. and strong RoL countries. We conclude that the lack of quarterly filing rules does not explain the lower restatement rate of foreign firms.

V. ADDITIONAL ANALYSES

IFRS Adoption in the Home Country and Likelihood of Restatements

The RoL measure is a static variable and does not consider changes in institutions over time. In this section, we use the mandatory adoption of IFRS as a proxy for time-series changes in the quality of accounting institutions in the home country.[27] Prior research suggests that IFRS adoption is associated with higher reporting quality (Barth, Landsman, and Lang 2008) and a significant reduction in information asymmetry (Daske, Hail, Leuz, and Verdi 2008; Wahid and Yu 2014). Further, the variation in whether and when different countries adopt IFRS allows us to examine the IFRS effect controlling for other concurrent changes affecting all foreign firms.

We examine the changes in the association between EM and restatement likelihood post-IFRS for foreign firms in weak and strong RoL countries.[28] We obtain from Daske et al. (2008) the list of countries that adopt IFRS and their adoption years. We estimate Equation (4) with two additional terms. We include a dummy variable (*Post IFRS*) for the years after IFRS and an interaction term (*EM Index* × *Post IFRS*), which is our main variable of interest examining the marginal increase in the association between EM and restatement likelihood following IFRS.

Table 7 presents the results for foreign firms from weak and strong RoL countries. We find that the effect on the *Post IFRS* variable is negative and significant only for the strong RoL firms (Coeff. = −1.699, p-value = 0.028). For the weak RoL firms, the estimated coefficient is negative, but not significant. The lower restatement rates following IFRS adoption may suggest fewer irregularities in the reported financials, but may also suggest a lack of detection of existing accounting irregularities. We next examine the extent of stringent detection triggered by IFRS adoption by evaluating association of restatements to the EM index following IFRS adoption.

The interaction term *EM Index* × *Post IFRS* examines whether the association between EM and restatement likelihood increased after IFRS adoption. Table 7 shows that the coefficient on the interaction term is positive and significant (Coeff. = −0.561, p-value = 0.04) for only the firms from weak RoL countries. This is not surprising since for the strong RoL firms, the association between EM and restatement likelihood was not significantly different from that of U.S. firms in Table 6.

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[27] Many foreign firms in our sample use U.S. GAAP. To the extent that home country IFRS adoption improves the quality of domestic accounting institutions, we expect IFRS adoption to improve outcomes even for U.S. GAAP users.

[28] We cannot conduct this test using *ICMW* because the ICMW sample begins only after 2007, reducing the variation in IFRS adoption that mostly occurred before 2007. The majority of the IFRS adopting countries first mandated IFRS in 2005.

TABLE 7

Time-Series Changes in Home Country Enforcement and Restatement Likelihood Mandatory Adoption of IFRS 2000–2010

	Foreign Firms: Weak RoL	Foreign Firms: Strong RoL
EM Index	**−0.159**	**0.064**
	[0.112]	**[0.351]**
EM Index × Post IFRS	**0.561****	**0.318**
	[0.040]	**[0.102]**
Post IFRS	−1.300	−1.699**
	[0.263]	[0.028]
F-test: *EM Index + EM Index × Post IFRS*		
F-stats	**2.28**	**4.19**
[p-value]	**[0.131]**	**[0.041]****
Firm Controls		
Size	−0.099	−0.041
	[0.161]	[0.521]
Leverage	0.541	0.121
	[0.445]	[0.835]
ROA_Current	−1.663	−2.048***
	[0.207]	[0.005]
ROA_Lagged	0.041	0.148
	[0.975]	[0.861]
Book-to-Market	0.183	0.146
	[0.545]	[0.490]
Big Five Auditor	−1.233***	0.460
	[0.000]	[0.118]
Analyst Coverage	0.051**	0.012
	[0.039]	[0.438]
Institutional Ownership	−0.717	0.414
	[0.205]	[0.205]
Sales Growth	0.266	−0.090
	[0.432]	[0.749]
Segments	−0.021	0.073
	[0.932]	[0.725]
Country Controls		
Accounting Difference	0.958	1.331**
	[0.362]	[0.032]
Country Market Cap	0.000	−0.000
	[0.254]	[0.542]
Country GDP Growth	5.537	−2.550
	[0.323]	[0.665]
Auditor Liability	0.703	0.160
	[0.559]	[0.704]
# Obs.	2,885	4,194
Pseudo R^2	0.145	0.079
Year FE	Yes	Yes
Industry FE	Yes	Yes

(continued on next page)

TABLE 7 (continued)

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.
This table reports the estimation from a logistic regression of Equation (4) after including a dummy variable (*Post IFRS*) for the years after IFRS was adopted in the firm's home country, and an interaction term (*EM Index* × *Post IFRS*). We obtain from Daske et al. (2008) the list of countries and years that mandatorily adopt IFRS (*Post IFRS*). The dependent variable $Restatement\ Irregularities_{i,t}$ is an indicator variable that takes a value of 1 if a firm reported an irregularities restatement for year *t*, and 0 otherwise. We define accounting irregularities as restatements that are subsequently followed by an internal or external investigation (Hennes et al. 2008). We use the country sample median of the rule of law index (= 1.64) to classify firms as from strong or weak rule of law countries. The rule of law index is from the Worldwide Governance Indicators created by the World Bank (Kaufmann et al. 2003) and used in La Porta et al. (2006). Coefficient estimates and p-values [in brackets] are from seemingly unrelated regressions of restatement probability on *EM* and other controls. F-tests test for the sum of the two coefficients, *EM Index*, and the interaction term (*EM Index* × *Post IFRS*). Estimates of the coefficients in the F-tests are shown in bold. Standard errors are clustered at the firm level. p-values are based on t-tests for differences in mean.
All other variables are defined in Appendix A.

The finding suggests that IFRS adoption had a greater impact on improving the reliability of restatement reporting for foreign firms from weaker RoL countries.

However, we note that even after IFRS adoption, there remain significant differences in the overall *Restatement-EM* sensitivity between the strong and weak RoL firms. Despite the incremental increase in the weak RoL firms following IFRS adoption, the F-tests in Table 7 show that the sum of *EM Index* and *EM Index* × *Post IFRS* is positive and significant for only the strong RoL firms (F-stat = 4.19, p-value = 0.041), while insignificant for the weak RoL firms (p-value = 0.131). Taken together, the results suggest that while IFRS adoption in the home country led to an incremental increase in the reliability of restatement reporting, the home country effect continued to affect the reporting behavior of all foreign firms even after IFRS adoption.[29]

The Restatement Disclosure Method Choices of Foreign Firms Listed in the U.S.

Prior studies show that, conditional on reporting a restatement, firms make disclosure choices regarding the announcement medium to minimize the cost of restating (Files et al. 2009; Myers, Scholz, and Sharp 2010; Badertscher and Burks 2011). While our main focus is on the likelihood of restatement, we also examine whether foreign firms differ in the disclosure method.

All firms are required to correct previous errors in their financials in their regular filings (10-K, 10-Q, 20-F, or 40-F). In addition to the regular filings, the SEC requires the restatements that are deemed "material" to be separately disclosed using a Form 8-K (or 6-K for foreign firms) (SEC 2004). Many firms also voluntarily issue press releases describing the error and its financial impact. Restatements announced using additional disclosure media (using an 8-K, 6-K, or press release) are presumably more visible and draw investor attention on a timely basis. In contrast, restatements that are considered immaterial are reported only in regular filings, which are less visible and available at the next regular filing date.[30]

[29] Prior studies find an increase in the number of restatements since the passage of SOX (Burks 2011). We examine whether SOX led to changes in the association between EM and restatement likelihood. In untabulated analysis, we find that while the association between restatement and EM increased following SOX, there are no significant differences across firms from weak versus strong rule of law countries.

[30] However, there could be cases where the next regular filing (e.g., 10-K) is the speediest form of communication—e.g., if the next 10-K is scheduled within two weeks. An 8-K filing is still necessary to indicate that prior financial statements should not be relied upon (see Taub 2012).

We examine opportunism in restatement disclosure by examining the association between the restatement's materiality and the use of a visible disclosure medium. If foreign firms are opportunistic, they will be less likely than the matched U.S. firms to report material restatements using visible disclosure media. We test this prediction using the following model:[31]

$$\begin{aligned} \mathit{Visible\ Restatement}_{i,t} = \beta_0 + \beta_1 \mathit{Materiality}_{i,t} + \beta_{2\text{-}13} \mathit{Firm\ Controls}_{i,t-1} \\ + \beta_{14\text{-}16} \mathit{Country\ Controls}_{i,t-1} + \varepsilon_{i,t}. \end{aligned} \tag{5}$$

The dependent variable, *Visible Restatement*$_{i,t}$, equals 1 if the restatement is reported with a separate filing (e.g., an 8-K or a press release) in addition to the regularly scheduled financials, and 0 otherwise (Files et al. 2009). *Materiality* is an indicator variable that equals 1 if the restatement is material, and 0 otherwise. We follow prior literature and gauge materiality using both quantitative and qualitative measures. We first define material restatements as those that have a greater than 5 percent net income impact (Nelson, Elliott, and Tarpley 2005). We also consider qualitative measures and define restatements as material if there is likely to be an intent (Acito, Burks, and Johnson 2009). We measure intent based on restatements (1) where the error changes a loss into profit, (2) that reverse an increase in earnings trend from prior years, (3) are issued in periods of high financial distress, defined as firm-years in the highest leverage decile, or (4) related to the core accounts.

While the SEC limits the use of less visible disclosure for restatements deemed material, not all foreign firms are subject to this requirement.[32] To ensure that our findings are not driven by differences in disclosure requirements between U.S. and foreign firms, we limit our foreign firm sample to those that file 10-Ks and, therefore, are subject to reporting requirements identical to those of U.S. firms.[33] We also control for the firm's tendency to issue press releases using the average annual number of press releases issued by the firm, starting from the first year the firm appears in our sample up to the current year (*Previous Press Releases*).

Table 8 presents the results of estimating Equation (5) as a SUR model to compare the coefficient estimate for foreign firms and their U.S. matched sample. The main variable of interest is the coefficient on materiality (β_1). Results show that the association between materiality and visible disclosure varies systematically across the foreign and U.S. samples.[34] Column (1) shows that the β_1 coefficient is positive and statistically significant only for the U.S. sample and insignificant for the foreign sample. The difference in the coefficients, however, is not statistically significant. In column (2), we consider qualitative measures of materiality. For foreign firms, the association between materiality and visible disclosure is insignificant, while for the U.S. sample, the association is positive and statistically significant (Coeff. = 1.691, p-value = 0.005). The difference between the two is statistically significant (p-value = 0.041), suggesting that the use of a visible disclosure medium for qualitatively material restatement is greater for U.S. firms.

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[31] Given the small number of observations for this test, we do not include industry and year fixed effects.

[32] For example, foreign firms filing a 20-F or 40-F are not required to file 8-Ks. These firms instead furnish current reports on form 6-K for timely disclosure of a material event. Unlike *filed* 8-Ks, which hold the preparer liable for any false or misleading information, 6-Ks are *furnished*, holding the preparer liable only when the preparer is proven to have "intentionally" provided false or misleading information. Filed information is subject to the liability provisions of Section 18 of the Exchange Act of 1934 and is automatically incorporated into issuers' registration statements. Furnished information is not subject to the same liability section and is not automatically incorporated into the registration statement, unless the issuer specifically requests its incorporation.

[33] Foreign firms that list directly on U.S. markets or that do not qualify for foreign-issuer status have to file their financials using the same forms as U.S. firms.

[34] In untabulated multivariate analysis, we examine the intercept effect. The difference in the likelihood of visible restatements for U.S.-listed foreign firms and the matched U.S. firms is not statistically significant (p-value = 0.870).

TABLE 8

Disclosure Choices and Home Country Rule of Law Conditional on Reporting Restatements 2000–2010

Model: $$Visible\ Restatement_{i,t} = \beta_0 + \beta_1 Materiality_{i,t} + \beta_{2\text{-}13} Firm\ Controls_{i,t-1} + \beta_{14\text{-}16} Country\ Controls_{i,t-1} + \varepsilon_{i,t}. \quad (5)$$

	(1)		(2)	
	All Foreign Firms	**U.S. Matched Firms**	**All Foreign Firms**	**U.S. Matched Firms**
	Quantitative (< 5% of NI)		**Quantitative and Qualitative**	
Materiality	**0.221**	**1.590****	**0.189**	**1.691*****
	[0.704]	**[0.025]**	**[0.649]**	**[0.005]**
F-test				
F-stats	2.23		4.16	
[p-value]	[0.135]		[0.041]**	
Firm Controls				
Size	0.091	−0.167	0.103	−0.010
	[0.672]	[0.453]	[0.626]	[0.955]
Leverage	0.402	4.380**		
	[0.690]	[0.016]		
ROA_Current	1.222	0.066	1.217	−0.388
	[0.421]	[0.983]	[0.397]	[0.880]
ROA_Lagged	0.881	3.155	0.934	3.504
	[0.488]	[0.450]	[0.451]	[0.305]
Book-to-Market	−0.314	1.210*	−0.335	0.973
	[0.492]	[0.072]	[0.456]	[0.141]
Big Five Auditor	−0.784	−0.146	−0.818	−0.196
	[0.350]	[0.847]	[0.339]	[0.796]
Analyst Coverage	0.002	−0.076	−0.000	−0.089
	[0.980]	[0.275]	[0.999]	[0.163]
Institutional Ownership	0.083	−0.036	0.108	0.349
	[0.873]	[0.967]	[0.837]	[0.687]
Sales Growth	−0.222	−0.625	−0.258	−0.646
	[0.632]	[0.586]	[0.585]	[0.405]
Segments	0.329	0.315	0.394	0.216
	[0.480]	[0.643]	[0.374]	[0.709]
Reporting Standard	0.000	0.000	0.000	0.000
	[0.864]	[0.272]	[0.780]	[0.418]
Previous Press Releases	−25.207**	17.286	−26.437**	19.957
	[0.021]	[0.426]	[0.015]	[0.378]
Constant	−1.561	−4.923*	−1.621	−4.347*
	[0.330]	[0.056]	[0.328]	[0.073]
# Obs.	118	93	118	93
Pseudo R^2	0.181	0.286	0.184	0.228
Country controls in Table 4	Yes	Yes	Yes	Yes

(continued on next page)

TABLE 8 (continued)

	(1)		(2)	
	All Foreign Firms	U.S. Matched Firms	All Foreign Firms	U.S. Matched Firms
	Quantitative (< 5% of NI)		Quantitative and Qualitative	
Year FE	No	No	No	No
Industry FE	No	No	No	No
F- test:				
F-stats	2.23		4.16	
[p-value]	[0.135]		[0.041]**	

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test. This table presents a seemingly unrelated logistic regression of the likelihood of stealth restatements on restatement materiality. *Visible Restatement*$_{i,t}$ equals 1 if the restatement is reported with a separate filing (e.g., form 8-K, 6-K, or a press release) in addition to the regularly scheduled financial statements, and 0 otherwise (Files et al. 2009). *Materiality* is an indicator variable that equals 1 if the restatement is considered a material restatement, and 0 otherwise. *Materiality* is measured using quantitative measures in Model (1), based on those that have greater than 5 percent net income impact. In Model (2), we consider qualitative measures and consider restatements to be material (1) where the error changes a loss into profit; (2) where they reverse an increase in earnings trend from prior years; (3) when they are issued by a firm in periods of high financial distress, measured as the firm-years in the highest leverage (liability divided by equity) decile; or (4) when they are related to the core accounts. F-tests compare the coefficients of the *Materiality* variable for the all foreign firm sample and their U.S. matched sample. Estimates of the coefficients included in the F-test are shown in bold. Standard errors are clustered at the firm level.

VI. CONCLUSION

We study restatements by U.S.-listed foreign firms, compare the extent of restatements by these firms with that of domestic U.S. firms, and examine the role of home country characteristics in the likelihood of the foreign firms issuing restatements. On the one hand, restatements reflect weakness in financial reporting. On the other hand, a restatement announcement implies that the accounting error or irregularity was identified and corrected, indicating that internal and external governance mechanisms (such as internal controls and external audits) performed their expected roles. Results in the paper suggest that foreign firms listed in the U.S. restate less than comparable U.S. firms despite the foreign firms having weaker accounting quality. The results suggest that the difference can be attributed to avoidance of reporting restatements. The difference with U.S. firms is driven primarily by firms from countries with weaker legal institutions.

Our results suggest that foreign firms listed in the U.S. are subject to less rigorous monitoring and enforcement than are domestic U.S. firms. Further, weaker institutions in the firm's home country reduce compliance with U.S. reporting rules for foreign firms accessing U.S. markets, despite a common set of U.S. rules that apply to them upon a U.S. listing. Therefore, compared to U.S. firms, restatements are a less accurate measure of reporting problems for non-U.S. firms.

The finding that foreign firms show less reliable detection and reporting of errors and irregularities raises concerns for investors and regulators. Restatements are an admission by the company of errors or irregularities in financial statements. Therefore, they are valuable to investors and regulators as an information mechanism for holding managers accountable for misreporting. Thus, a lack of reliable restatement reporting increases the cost for investors and regulators to monitor firms. The broader implication is that the lower frequency of restatements implies fewer *ex post* penalties, which reduces the *ex ante* discipline in financial reporting.

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APPENDIX A

Variable Definitions

Variable	Description
Earnings Management	
$EM1_{i,t,prior}$: *% of firm-years with a small positive income*	% of firm-years that have a small positive income. Small positive income is defined as years where net income scaled by total assets (all using unrestated numbers) falls between 0 and 0.01. For each firm-year, we calculate the percentage of years with small positive income using the last three years; the current fiscal year (*t*), the previous fiscal year ($t-1$), and two years before ($t-2$). The subscript $t-1$ denotes that the measure represents values *prior* to identifying the need to restate the year *t* financials.
$EM2_{i,t,prior}$: $\lvert Accruals \rvert / \lvert CFO \rvert$	The magnitude of total accruals, measured as the ratio of the absolute value of total accruals to the absolute value of operating cash flows (Leuz et al. 2003) in the current fiscal year *t*. The magnitude of the total accruals is used as a proxy for managerial discretion and scaled by operating cash flows to adjust for the differences in firm economics. Total accruals are calculated using the unrestated financials and defined as (Δ*Current Assets* $-$ Δ*Cash*) $-$ (Δ*Current Liabilities* $-$ Δ*Current Debt* $-$ Δ*Tax Payable*) $-$ Δ*Depreciation,* following Dechow et al. (1995).
$EM3_{i,t,prior}$: *Accruals Quality*	The standard deviation of the residual from a firm-level regression of current accruals on prior and future operating cash flow (Dechow and Dichev 2002; Wysocki 2009) using the unrestated financials. The measure captures the estimation errors in the accruals process by estimating how well accruals map into cash flows. The regression model is estimated cross-sectionally each year for each industry (two-digit SIC-code).
$EM4_{i,t,prior}$: $-Corr(\Delta Accrual, \Delta CFO)$	The correlation between changes in accruals and operating cash flows multiplied by -1. The negative correlation is a discretionary smoothing measure that proxies for management intervention over and above the natural level of accruals accounting (Francis et al. 2005). We use the unrestated figures and calculate the correlation between the changes in accruals and operating cash flows using the last three years.
$EM\ Index_{i,t,prior}$	Average percentile rank for each firm for the year across the four (or as many as are available) measures of earnings management in the current fiscal year. The subscript *prior* denotes that the measure represents values prior to identifying the need to restate the year *t*'s financials, using unrestated numbers. Each year, all firms are ranked on each measure and percentile rank is assigned to the firm for all four (or all available) measures. Higher values indicate higher earnings management.
Firm Characteristics	
$Size_{i,t}$	Natural log of total assets at the end of fiscal year *t*.
$Leverage_{i,t}$	Long-term and short-term debt, scaled by total assets at the end of fiscal year *t*.
$ROA_{i,t}$	Income before extraordinary items scaled by total assets at the end of fiscal year *t*.

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APPENDIX A (continued)

Variable	**Description**
Book-to-Market$_{i,t}$	Book-to-market ratio measured at the end of year *t*.
Big Five Auditor$_{i,t}$	Indicator variable equal to 1 if the firm is audited by one of the Big 5 audit firms for fiscal year *t*, and 0 otherwise.
Analyst Coverage$_{i,t}$	Number of analysts covering the firm at any point during fiscal year *t*.
Institutional Ownership$_{i,t}$	Percentage of float shares owned by U.S. institutional investors at the end of year *t*.
Sales Growth$_{i,t}$	Percent increase in sales from fiscal year $t-1$ to year *t*.
Segment$_{i,t}$	Natural log of the number of the firm's business segments for fiscal year *t*.
Reporting Standard$_{i,t}$	Indicator variable equal to 1 if firms use U.S. GAAP or IFRS without reconciliation for fiscal year *t*, and 0 if firms use local GAAP with reconciliation to U.S. GAAP.
ICMW$_{i,t,prior}$	Indicator variable equal to 1 if the firm reported an internal control material weakness (SOX Section 404) for year *t*, *prior to* identifying the need to restate the financials, and 0 otherwise. The subscript *prior* denotes that the measure represents values prior to identifying the need to restate year *t*'s financials. We use the original IC effectiveness report to focus exclusively on the ICMW prior to the discovery of year *t*'s restatement.
Previous Press Releases$_{i,t}$	Annual average of 8-K filings, measured over the time period starting with 2000 (or the earliest year the firm appears in the sample) to the year of the restatement.
Restatement Characteristics	
Restatement Irregularities$_r$	Indicator variable equal to 1 if the restatement is related to an accounting irregularity, and 0 otherwise. We define restatements related to severe accounting irregularities using (1) *ex post* measures using external or board investigation (Hennes et al. 2008), and (2) *ex ante* measures using the core/non-core account classification (Palmrose et al. 2004). Core accounts are those related to revenue recognition, cost of goods sold, operating expenses, or depreciation. The classification uses restatement descriptions provided in Audit Analytics.
Core Account$_r$	Indicator variable equal to 1 if the restatement is related to one of the following accounts; revenue recognition, cost of goods sold, operating expenses, or depreciation, and 0 otherwise (Palmrose et al. 2004).
Visible Restatement$_r$	Indicator variable equal to 1 if the restatement is reported with a separate filing (e.g., form 8-K or 6-K, a press release, or non-timely filings such as NT 10-K or equivalent) in addition to the regularly scheduled financial statements, and 0 otherwise. The 6-Ks of foreign firms often include other filings unrelated to restatements (e.g., quarterly results). We examine all 6-Ks that announce a restatement to determine whether the restatement announcement was issued alone, or in conjunction with quarterly results.
Magnitude$_r$	The dollar amount of equity restated, scaled by total assets.

(continued on next page)

APPENDIX A (continued)

Variable	Description
Materiality$_r$	Indicator variable equal to 1 if the restatement is deemed material, and 0 otherwise. We define materiality using quantitative measures—those that have a greater than 5 percent net income impact—and qualitative measures that capture the firms' intent—where the restatement (1) changes a loss into profit, (2) reverses an increase in earnings trend from prior years, (3) is issued in periods of high financial distress, defined as firm-years in the highest leverage decile, or (4) is related to the core accounts.
Time to Discovery$_r$	Number of months from the end of the restatement period to the day the restatement was reported/discovered.
Litigation$_r$	An indicator variable equal to 1 if there is an identified litigation related to the restatement within one year after the restatement announcement, and 0 otherwise.
SEC Investigation$_r$	Indicator variable equal to 1 if an SEC investigation relating to the restatement is identified by Audit Analytics, and 0 otherwise.
CEO Turnover$_r$	An indicator variable equal to 1 if the CEO leaves within a year of the restatement.
Country Characteristics	
Weak RoL$_c$	Indicator variable equal to 1 if the rule of law index is below the country sample median (= 1.64). The rule of law index is from the Worldwide Governance Indicators created by the World Bank (Kaufmann et al. 2003) and used in La Porta et al. (2006). Rule of law measures the extent to which agents have confidence in and abide by the rules of society in the year 2000. These include perceptions of the incidence of both violent and non-violent crime, the effectiveness and predictability of the judiciary, and the enforceability of contracts.
Accounting Difference$_c$	Measure of the difference between two local accounting standards from Bae, Tan, and Welker (2008) and modified in Wahid and Yu (2013). The measure is constructed based on a survey examining the extent to which local accounting standards deviate from U.S. GAAP for a list of 21 accounting rules (Andersen et al. 2001). Two rules are considered similar when the rules of both countries comply with U.S. GAAP. Two countries that follow local standards that are not compliant with IFRS are considered to have similar rules only if they derive from the same legal origin.
Country Market Cap$_{c,t}$	Market capitalization in US$ billion for country c at the end of year t, obtained from Standard & Poor's (2010) *Global Stock Markets Factbook*.
Country GDP Growth$_{c,t}$	Percent GDP growth in the prior year, from year $t-1$ to t.
Auditor Liability$_c$	Liability standard for accountant measure from La Porta et al. (2006).

i = firm, c = country, r = restatement, t = year.

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Book Reviews

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Stephen A. Zeff, Rice University, continues as the book review editor for the May issue of the journal.

Two copies of books for review should be sent to the incoming Book Review Editor: Professor Gary C. Biddle, Room 1305, K.K. Leung Building, The University of Hong Kong, Pokfulam, Hong Kong. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

FRANK CLARKE and GRAEME DEAN (with MATTHEW EGAN), *The Unaccountable & Ungovernable Corporation: Companies' Use-By Dates Close In* (London, U.K.: Routledge, 2014, ISBN 978-0-415-71912-4, pp. xiii, 177).

The takeaway from Frank Clarke and Graeme Dean's book, written with Matthew Egan, is a gloomy one, succinctly summarized in the first six words of the Epilogue: "It is hard not to despair" (p. 150). The authors' argument is—at its core—that the modern corporation has passed its "use-by date," not merely because of the problems illuminated by the Global Financial Crisis (GFC), but because of much deeper endemic and structural flaws in the corporation that predate the GFC. In the authors' view, these flaws include many of the central features of the corporation—unlimited life, limited liability, and open-ended purpose—and also a raft of accounting problems, particularly related to the disclosures of complex risks and group holdings.

The authors write from a global perspective, with a focus on recent experience in Australia. Their proposals are sweeping and radical, although the authors insist the recommendations would be feasible if there were only sufficient political will. These recommendations include: limiting the life of corporations; limiting the object of corporations; limiting, or in some situations removing, limited liability protections for shareholders; scrapping consolidation accounting for subsidiaries; and substantially empowering auditors and expanding audit rules to ensure more accurate financial statements. The authors' despair and the book's gloomy tone arise from a deep skepticism about whether there is sufficient political support for anything approaching the level of reform the authors believe is necessary. Thus, the authors conclude, they are writing "more in anguish than in anger" (p. 150).

The book is comprised of a Prologue, six chapters, and the aforementioned Epilogue. The Prologue frames the book in broad terms, connecting it to John Henry Newman's directive, 150 years ago, that "the function of a university was to expose false doctrines and profess the truth" (p. 2). The authors are seeking in this book to summarize their extensive academic and multidisciplinary writings and perspectives, and by doing so to profess the truth about recent business practices.

The authors are, admittedly and explicitly, not "out to win friends" (p. 8). Clarke is Honorary Professor of Accounting at The University of Sydney and *Emeritus* Professor at The University of Newcastle. Dean is *Emeritus* Professor of Accounting at The University of Sydney. Egan is Lecturer in Accounting at The University of Sydney. They explicitly state that much of the work and ideas in the book have been published previously elsewhere; indeed, significant portions of the book—shaded in grey in the text—are based on opinion pieces previously published by Clarke and Dean.

The first two chapters directly address the claim that the modern corporation has reached its "use-by date." The authors argue that corporations have become unaccountable and ungovernable. Chapter 1 argues that both *ex ante* and *ex post* approaches to corporate governance have failed. The *ex ante* approach has been inadequate, not only because of the proliferation of increasingly complex business practices, along with even more complex rules and requirements, but also because corporations have been embraced as "persons" and given the same rights as humans in various aspects. The *ex post* approach has been inadequate because of the dearth of meaningful sanctions and enforcement.

Chapter 2 critiques the role of the audit and accounting industry in corporate misstatements and misconduct. The essence of this chapter's argument will be familiar to anyone who knows Clarke and Dean's previous work. In particular, the authors focus on the "serviceability," or usefulness, of auditors' assessments of financial data: "[S]erviceability was suggested as the primary criterion underpinning assessments of whether data in financial statements showed true and fair representations of (or fairly present) financial position and financial performance" (p. 54). Of course, accounting data are not goods and services, but the authors argue that accounting nevertheless should be subject to some form of consumer-protection regime—as are most other aspects of the economy.

The authors link the failures of accounting to the more general problems of the modern corporation, arguing that accountability and the notion of the "true and fair" quality criterion underpin the basic benefits given to corporations in modern society. These fundamental accounting notions are a kind of prerequisite to the privilege of incorporation; it follows, then, that corporations that do not satisfy the "true and fair" quality criterion in presenting their financial statements should not be entitled to the protections afforded by corporate legislation.

The authors' approach varies between scholarly and mainstream, and the discussion frequently switches between academic theory and popular media. At times, the authors rely on primary sources, including their own work, but they also frequently rely on secondary, or tertiary, authority. For example, in Chapter 3, the authors criticize regulators and regulation, and suggest that the extant academic theories of industry capture and private interest are deficient and provide little normative guidance for systemic reform. However, instead of citing specific primary authority, either theoretical or empirical, the authors instead cite a recent PBS *Frontline* television program (p. 58), a recently completed doctoral dissertation (p. 60), a recently published book by *Financial Times* journalist, Gillian Tett (p. 61); and the recent film, *Inside Job* (p. 62). The authors' extensive reliance on such authorities makes the text more readable and relatable, and it connects the discussion to the popular media, but academics might find some of the discussion perhaps less serious than it might have been.

One of the most interesting segments of the authors' regulatory critique in Chapter 3 is their discussion of Australian corporate and financial law reform, particularly Australia's Company Law Economic Reform Program, known as "CLERP 9." The CLERP was a long-term, big-picture Treasury program that was implemented, beginning in 1997. CLERP was designed to cover a wide range of issues through reform papers, which received attention and public comment, and then ultimately were developed into legislative proposals. It was originally envisioned that there would be six such CLERP proposals, but there ended up being nine, over the course of 15 years.

CLERP 9, the ninth proposal, was published in 2002 "against a backdrop of extensive corporate accounting and auditing capers and market failures in Australia and elsewhere" (p. 69). CLERP 9 included several measures that were rooted in the post-1929 crash financial regulatory response, including a disclosure framework, expanded investor rights, and audit reform. The authors consider arguments about the extent to which CLERP 9 was responsible for Australia being relatively unscathed by the GFC.

The authors' conclusion is interesting. They find support for the notion that CLERP 9 was a "lighter" regulatory response than responses by other regulators, including the adoption of the Sarbanes-Oxley legislation in the U.S. They also criticize Australian regulators for overlooking a range of non-bank corporate problems, and they particularly target the Australian Securities and Investments Commission (ASIC). The authors conclude that, although ASIC has been perceived as an effective regulator, perception does not match reality. In fact, the authors argue, ASIC has been inept and slow to respond to problems, and has delivered only slap-on-the-wrist penalties in the relatively rare instances when penalties are imposed at all. The overall message is that the Australian markets, and Australian regulators, were fortunate not to have suffered more during the GFC.

Chapter 4 covers the daunting task of performing effective company audits. Clarke and Dean have covered some of this territory before, in their book *Indecent Disclosure* (Clarke and Dean 2007), and their criticisms about the inherent uncertainty in standards-based accounting practice have only become more pointed since then. Overall, the authors label the current approach to accounting practices based on International Financial Reporting Standards (IFRS) as "a mission impossible" (p. 84). The central problem, in the authors' view, is not the conflicts or problems in auditing processes, but rather the inherent defects in the accounting data combined with the straitjacketing dictates of accounting rules. The authors argue that the mandatory audit and accounting standards approach is the antithesis of a professional regime and accordingly "*hobbles* auditors' activities" (p. 84; emphasis in original).

The authors support their argument with case studies in which the professional judgment of auditors should have, or would have, exposed material misstatements or even fraud. Readers who are not familiar with prominent examples from Australia will likely find the discussion of these examples fresh and interesting. For example, the authors cover ASIC's high-profile—and failed—case against telecommunications corporation, One.Tel, which allegedly misclassified long-term liabilities as short-term liabilities. Among the culprits that emerge from this section are the non-executive directors, who allegedly knew important information related to One.Tel's insolvency, but later appeared to be "dogged by very poor memories" (p. 86). Other examples include the 2001 collapse of HIH, the allegedly conflicted transactions at Parmalat, and the Centro-PwC affair. These discussions are substantially based on the authors' previously published work.

Chapter 5 is a summary critique of executive compensation disclosure. The authors criticize voluntary reductions in executive pay as inadequate. There are a few interesting pages devoted to the 2010 remuneration changes in Australia, and other more recent proposals requiring additional disclosures, including disclosure about the extent of clawbacks and a general description of a company's governance framework for compensation. The tone of this chapter is highly critical of executive compensation, as is evidenced by its title: "Executive Snouts in the Trough."

Chapter 6 covers group accounting. Here, the authors provide support for their radical proposal to eliminate the current form of conventionally prepared consolidated financial statements. It is uncontroverted that group accounts have become substantially more complex in recent years, and few readers will disagree with the authors' conclusion that the difficulties are due in large part to "off-balance sheet activities, such as complex, intra-group financing and other transactions" (p. 124).

As the authors note, one central notion in considering the reform of the consolidation approach is the question of control. The authors criticize recent IFRS reforms that were designed to tighten reporting requirements for subsidiaries and special-purpose vehicles, particularly IFRS 10, released in May 2011, which proposed strengthening the conventional control criterion for consolidation. The authors are skeptical that recent changes have improved disclosure or are useful for investors seeking to understand business risks.

As in other chapters, the authors cite prominent relevant Australian cases, including the 1963 failure of electrical retailer H. G. Palmer, whose accounting manipulations are compared to those at Enron, and the Patrick Stevedores/MUA waterfront dispute of 1998. The examples provide historical context and reinforce the conclusion that the problems with group accounting are not new.

Most usefully, the authors include a statistical example of a financial position based on their proposed approach to group accounting. In the example, "H Ltd has a 75% interest in A Ltd and a 60% interest in B Ltd, while A Ltd has a 20% interest in B Ltd" (p. 142). The authors then illustrate how financial statements could be prepared to show positions and performance for each of H, B, and A. The proposed approach would automatically calculate the various fractional interests, and the parent company's statements would continuously present a contemporaneous representation of its investments in any controlled entities.

The proposal raises numerous interesting questions. The authors assert that no additional financial statements would be necessary, because the statistical disclosures would capture the relevant information. However, current approaches include more detailed financial information, and also cover some subsidiaries, including some variable interest entities (VIEs), which presumably would not be consolidated under the authors' proposal (because the parent would not satisfy the control criteria). A single financial statement might be more accurate, but might also involve the disclosure of less important information. Disclosure under the proposed regime would be particularly challenging for large, complex financial institutions, including banks, that have huge numbers of VIEs and VIE-related transactions, some of which are consolidated and some are not (and where it would be unclear whether the control criteria would be satisfied).

Nevertheless, the authors have advanced an important critique, and have put on the table a proposal that is worthy of debate. The proposal also dovetails with the critique by Andrew Haldane, who has demonstrated the increasing complexity of financial disclosures and practices, and has advocated greater simplicity.

Clarke and Dean have spent their academic careers critiquing the unaccountable and ungovernable corporation. With the assistance of Egan, they have put in one place a compilation of their variously expressed views. Many of the arguments in this book are not new—Clarke and Dean have been making them for decades—and some scholars will find the book's tone strident. Moreover, many scholars will disagree about whether the book has fulfilled John Henry Newman's directive and exposed false doctrines or professed the truth. Nevertheless, although the authors' diatribes are unlikely to change many scholars' minds about the problems facing modern corporations, the authors have, with this book, placed an important historical marker—which others will either accept or reject—summarizing their critique of the modern corporation, and warning that if regulators and legislators do not heed their warnings, the markets will be less efficient and fair, and more vulnerable to corporate collapses and financial crises.

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KUNIO ITO and MAKOTO NAKANO (editors), *International Perspectives on Accounting and Corporate Behavior* (Tokyo, Japan: Springer, 2014, ISBN 978-4-431-54791-4, pp. ix, 335).

Among efforts to globalize rules and regulations governing commerce, the IFRS project is a leader—ahead of initiatives to harmonize across countries' securities laws, labor laws, immigration laws, and product-safety laws. As of 2014, more than 120 jurisdictions across the world have some sort of IFRS harmonization program in place, according to Deloitte's IAS Plus (http://www.iasplus.com). The study of this bold project—to create one set of accounting rules for the world—thus has implications not only for our understanding of financial-reporting phenomena, but also for our understanding of the globalization of business more generally.

As ambitious and impressive as the rapid proliferation of IFRS is, academic research has begun to uncover that IFRS harmonization is a deeply local process—rooted in the economics, politics, and culture of the jurisdictions in which it is being considered (see e.g., Ball 2006; Leuz 2010; Ramanna 2013). As countries embrace IFRS in varying degrees, their local institutions adapt to or resist the imported standards, sometimes yielding unintended consequences. The more we understand this globalization at the local level, the better equipped we are to effect future efforts at international harmonization.

In the vein of the arguments above, Kunio Ito and Makoto Nakano have edited a thought-provoking volume on issues related to IFRS harmonization in Japan. The book is perhaps immodestly and inappropriately titled "*International* Perspectives on Accounting and Corporate Behavior" (emphasis added), because it is mostly about Japan, but it nevertheless brings to the table a large and institutionally rich body of evidence that can augment our understanding of what IFRS harmonization really means.

Japan is an interesting setting in which to study the local impact of IFRS harmonization, not least because the Japanese model of capitalism is so different from the Anglo-American model, which implicitly forms the basis for much of IFRS. The book provides a wealth of detail on the Japanese model of capitalism and its implications for corporate governance and accounting policy. Unfortunately, much of this detail is implicit rather than explicit in the writing—so to truly appreciate the book's value, readers must be prepared to do some of the heavy lifting themselves. At the risk of oversimplifying, I provide below a brief summary of the Japanese model that emerges from a careful reading of the book.

The key feature of the Japanese model is that it is stakeholder-focused, rather than shareholder-focused. One way to appreciate this distinction is to recognize that there are circumstances under which Japanese managers will prioritize the interests of their employees, suppliers, customers, and even the country as a whole over the interests of their shareholders. Another way to appreciate the distinction is to understand Japanese managers as being "relational" rather than "transactional" in their dealings with various constituents. What matters most in these contexts is preserving relationships and, just as importantly, *being seen* as preserving relationships. Put differently, social capital and social standing are characteristic features of the Japanese corporate manager—and a theory of the firm in Japan must take these features into account, in addition to considering the neoclassical Western view of the manager as a self-interested being who responds largely to monetary incentives.

The description of Japanese capitalism above raises two questions: How does the modern Japanese manager insulate himself (yes, this cadre is largely male) from the vicissitudes and pressures of the capital markets? Don't equity analysts, short sellers, hedge funds, and similar entities pressure these managers to focus on the bottom line, as they are generally seen as doing in American capital markets? After all, isn't Japan part of the international market system? Part of this insulation from capital-market pressures, the book argues, is provided by relatively low levels of transparency in financial reporting and corporate governance practices in Japan. The culture around corporate reporting is focused on showcasing stability and persistence rather than timely disclosure of news, good or bad. These features of the Japanese system, the book argues, allows for managers to focus on long-term decision making rather than on short-term earnings benchmarks. The result, they say, is corporations that last longer and deliver more consistent earnings performance.

If the model presented above seems excessively rosy, then the reader is left hoping for more. The book does not adequately present what is likely the flip side of this low-transparency, relationship-driven model of capitalism. For while the Japanese model might be good at orienting managers to the long-term—and encouraging them, for example, to preserve the dignity of employee relationships by avoiding transient layoffs—it can also provide insulation from the dynamism of capital markets that encourages the kind of creative destruction and innovation that are the catalysts of economic growth. After all, Japan has now endured more than two decades of economic stagnation, despite several well-intentioned efforts to jumpstart the economy.

The lack of transparency in the Japanese model has even more worrisome implications. It can insulate corruption and fraud. Take, for example, the recent and prominent case of Olympus Corporation—the Japanese optics manufacturer with a global presence. In Fall 2011, Olympus made Michael Woodford, a British national, the firm's CEO—the first non-Japanese to serve at the helm of the then 92-year-old Japanese company. Soon after assuming senior management responsibilities, Woodford became aware of questionable accounting practices at the firm. He found evidence suggesting that the company had engaged in a fraudulent merger—paying large sums as consulting fees to acquire what seemed like a shell company with no assets or operations.

Woodford suspected that the merger was being used to cover up past losses at the company, a practice not entirely uncommon in Japan. Since the Japanese stock-market and real-estate bubbles had burst in the early 1990s, several Japanese companies had been saddled with considerable losses. But many of these companies were reluctant to recognize these losses at once, as would be done under conservative accounting principles. After all, as discussed earlier, there was an expectation that the managers show stability and persistence in earnings. So, several companies adopted an ingenious way of gradually amortizing these losses. They sold impaired assets to third-party buyers at inflated prices and then compensated the buyers by paying high ongoing "advisory fees." This practice is known in Japanese as *tobashi* or "blowing away"—a euphemism for gradually blowing away losses.

Woodford suspected a form of *tobashi* at Olympus and, taking a page out of the standard Anglo-American corporate governance manual, went public with the concerns. What happened next was telling. Olympus' board fired Woodford barely two weeks into his new role. The board chairman, Tsuyoshi Kikukawa, who was also Woodford's predecessor as CEO, explained the firing by arguing that Woodford "was unable to understand that we need to reflect . . . Japanese culture [in our decisions]" (Tabuchi 2011a). Kikukawa added of Woodford, "I don't think he liked Japan" (Tabuchi 2011b).

The international outcry that followed Woodford's firing eventually led to a fuller investigation at Olympus. The investigation uncovered that the suspicious merger that initially tipped off Woodford was part of a series of fraudulent transactions dating back to the 1990s. The transactions had been initiated to gradually amortize losses, so as not to affect the company's ability to meet its various expectations to suppliers, customers, and employees and so as to preserve the company's reputation in Japanese society.

Thus, the Japanese model of capitalism that is at the heart of the various studies in the Ito-Nakano book is one of contradictions. This is what I refer to as the "paradox of Japanese capitalism" (Ramanna and Shaffer 2012). The Japanese model is implicated in more than two decades of economic stagnation, yet it has enabled relatively low levels of unemployment and poverty. While Japanese companies are perceived as organizationally rigid and hierarchical, they have provided relatively high levels of job security for employees. Despite an economy in relative decline since the 1990s, Japan has enjoyed a low crime rate and high social cohesion. There are merits to the Ito-Nakano framework of low levels of corporate transparency enabling greater corporate stability and persistence—but not without attendant costs.

Enter into this already complex environment, IFRS. Japan, like most large economies in the world, has an active program of IFRS harmonization. But Japan's embrace of IFRS has not been accompanied by a corresponding embrace of the regulatory and enforcement institutions that are implicit in IFRS. The result is illustrated in several of the 13 chapters, all written by Japanese authors, of the Ito-Nakano book.

As is the case with volumes of collected essays in general, it is difficult to do justice in a brief review to the many individual studies that make up the Ito-Nakano book. The book makes several important points—not least that IFRS, with its focus on the asset and liability view, may be ill-suited to Japan's manufacturing-based economy, where an income-based view seems more sensible. Below I discuss a few of the studies, particularly highlighting how they relate to the preceding discussion on the Japanese model.

Yusuke Takasu and Makoto Nakano's chapter, "What Do Smoothed Earnings Tell Us About the Future?" presents intriguing evidence on the "bright side" of earnings smoothing. In a sample of Japanese non-financial firms spanning 20 years through 2010, the authors find that income smoothing is related to future earnings persistence and that firms engaging in more smoothing tend to have more stable dividend payouts in the future. The authors seem to interpret this evidence as favoring the low-transparency of the Japanese model, which enables such income smoothing.

In "The Effect of Accounting Conservatism on Corporate Investment Behavior," Souhei Ishida and Kunio Ito make equally intriguing, although less compelling, arguments about the role of accounting conservatism in the Japanese model. They make the distinction between conditional and unconditional conservatism, arguing that the latter, by understating assets, provides more "slack" and mitigates downward earnings volatility. The result, they argue, is greater incentives for risk-averse Japanese managers to take on risky investment projects. By contrast, conditional conservatism, by forcing managers to recognize losses in a timely manner, creates disincentives for taking on risky projects. Risk-averse managers, anticipating the reputational and relational penalties of recognizing losses on receiving bad news, will forgo risky investment altogether, they claim.

There are striking parallels between the arguments above and the Olympus case discussed earlier. Indeed, as the Ishida-Ito chapter argues, it appears that at least some Japanese managers are wary of recognizing timely losses because of the social consequences of doing so. Whether anticipation of these negative social pressures is powerful enough to cause them to forgo risky investments altogether is a different matter, one on which the evidence is more mixed. After all, Olympus and the many other Japanese firms that engaged in *tobashi* would not have had to do so were it not for their speculative investments in equities and real estate that subsequently turned sour.

A collective interpretation of the Takasu-Nakano and Ishida-Ito chapters yields some interesting observations on preferences for accounting properties in the Japanese model. The Japanese model seems to appreciate the advantages afforded by having flexible *matching* principles to enable earnings smoothing. The model also has an appreciation for the high *verifiability* thresholds that underlie unconditional conservatism—because of the "slack" imparted to balance sheets as a result. But the model has little taste for *conditional conservatism,* because losses generated as a result can put managers in an awkward position *vis-à-vis* their relational commitments to various of the company's stakeholders. In the economic theory of accounting, matching, verifiability, and conditional conservatism all go hand in hand. Each serves as a complement to and a check on the other, enabling collectively a balance between relevance and reliability that makes financial reports decision-useful (see, e.g., Kothari, Ramanna, and Skinner 2010). That the Japanese model seems to selectively embrace some but not all of these properties is a conceptual red flag and a potential explanation for the low levels of international confidence in Japanese financial reporting.

Several chapters in the Ito-Nakano book examine more explicitly the economic consequences of IFRS harmonization in Japan. For instance, Yukari Takahashi's chapter, "Accounting Policy Choice for Negative Goodwill," has implications for the elimination from Japanese GAAP of negative-goodwill amortization. (The elimination is due to IFRS convergence efforts.) The Takahashi study investigates how firms chose the amortization

period on their negative goodwill when this choice was available to them. The author concludes that this discretion was used by firms to enable better matching in earnings and therefore that the elimination of negative-goodwill amortization compromises the properties of earnings. This study, while intriguing, does not convincingly rule out the possibility that the amortization discretion was being also used opportunistically to smooth earnings. The merits of timely recognition of extraordinary gains from negative goodwill are not fully considered.

The final chapter that I will discuss here is Yuki Tanaka's study of corporate environmental disclosure reports, "The Effect of Continuous Disclosure of Environmental Reporting." The study is intriguing because, although such reports are voluntary, a seemingly large proportion (about 50 percent) of Japanese firms above a certain size issues them. This high disclosure rate is at odds with the low-transparency model of Japanese capitalism discussed earlier. Tanaka and the book's editors conjecture that companies issue environmental reports as a signal of their "self-discipline" on other corporate governance matters. To marshal evidence for this claim, the study documents a negative association between the incidence of such reports and a firm's cost of capital.

Given Japan's stakeholder model of capitalism discussed earlier in the book, it is not clear how meaningful a cost-of-capital study is in the Japanese context. Also, are the methods of estimating cost of capital developed for Anglo-American style capital markets valid in the Japanese context? The Tanaka study does not satisfactorily consider these issues, which in turn casts some doubt on the interpretation that environmental disclosure in Japan is a signal of corporate self-discipline. An equally plausible explanation is that the environmental reports are a form of consumption for companies that otherwise enjoy preferential access to capital in Japan's notoriously cozy financial sector. Tests to discriminate between these explanations will make future studies of this nature more compelling.

The first decade of the IASB witnessed remarkable growth in the adoption of IFRS across the world. By contrast, IFRS adoption in the second decade has been more modest. Leaders of the global accounting standard-setting community are now confronted with the question of how to sustain momentum for IFRS in an increasingly multipolar and geopolitically volatile world. Answering this question will likely involve a deeper appreciation of the ground realities of IFRS adoption, including the local costs of IFRS enforcement. Books such as the one by Ito and Nakano are critical instruments in building this appreciation. Scholars and practitioners in the IFRS harmonization space will be well served by obtaining a copy of the Ito-Nakano book for their institutional libraries.

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EMMANUEL OKAMBA, *La comptabilité fondamentale* [*Basic Accounting*], *Collection recherches en gestion* (Paris, France: L'Harmattan, 2014, ISBN: 978-2-336-29128-4, pp. 216).

The title of the book is not very common, but it has already been used in French. The most common translation of *Basic Accounting* is *Financial Accounting*, but this phrase is less faithful to the substance of the discipline of Fundamental Accounting. In his book, Emmanuel Okamba wants to describe a field of knowledge that, according to him, was shaped by two schools of thought, i.e., Luca Pacioli's Venetian School on the one hand, and the American School of Watts and Zimmerman (1986) on the other. We do not really agree with the author on this issue: first, because between Luca Pacioli and Watts and Zimmerman five centuries have passed; and second, because the author does not seem to have read either the most important works in accounting theory, or the founding works of accounting epistemology. We believe that it is not superfluous to read Most (1977), Hendriksen and van Breda (1991), or Demski and Christensen (2002).

Fundamental Accounting, in our opinion, should focus on the deep structure of accounting, which was perfectly described by Mattessich (1957) and Ijiri (1967), among others. Mattessich also wrote a thorough study of the various accounting schools of thought (Mattessich 2006, 14–28), and so did Chatfield (1974). The title of the book does not really give information about its contents: the book deals with some fundamental concepts that could be used in accounting, which is different from genuine fundamental accounting, with double entry, which, in our view, can only be that of structured flows or defined as a vector space structure.

In the first part, the author makes an aesthetic analysis of performance and, for this, he examines the epistemological status of modeling in science and the universal measurement and evaluation systems used. The author makes some initial remarks on "the quantity" as mentioned in Aristotle's Organon, on the golden number and on the number Pi, and on the principle of double registration and the principle of causality. Even if these are interesting observations, they are not always relevant or true. Causality is not, in my opinion, one of the great features of accounting. Unlike in economics, the causality of accounting is poor, and its relationship with time, unlike with physics, is schematic. The author wishes to solve the following riddle: "How does proportionality enable us to explore performance in structured accounting and finance models?" The author also wants to use a normative approach to study the rationalities that structure accounting tools and methods in order to demonstrate the existence of a fundamental accounting equation, with genetic and aesthetic properties, and to draw the consequences on the reported modeling and the exploration of performance. These statements, which are the combination of ancient traditional knowledge and a modern philosophy of accounting, do not always appear compatible to me. The great problem of "What is accounting?" should have been investigated more thoroughly. Since he seems fascinated by Egyptian knowledge, whether it be esoteric or explicit, such as pyramids, pharaohs, the golden ratio, and the Egyptian anthropometric system, he should have started at the beginning, namely by using the work of indisputable researchers like Megally (1969, 1977).

Then he could have made the link between the ancient principles and the most modern principles of the late Middle Ages. The author is right to insist on proportionality and on the fundamental balance of accounting, but he forgets that what is fundamental in accounting is the vector space underlying structure, which explains both flows and stocks, and which allowed classical authors to establish essential bases: I have especially in mind authors like Barton (1975), De Morgan (1846), Ijiri (1967), Leontief (1986), Mattessich (1957), Moonitz (1961), Shank (1972), and Zannetos (1963).

The author concludes the first part of the book by saying that "From ancient Egypt to the work of Luca Pacioli, until today, the historical origins of accounting and finance show that the modeling and control of accounting and financial constructions involve the combination of arithmetic (the number Pi), geometry (measuring the golden number) and weight (realization of financial accounting and aesthetic constructions) to minimize the output and input movements and to achieve the ultimate result in a spatio-temporal cycle" (p. 147). In my view, Fundamental Accounting has nothing esoteric or cabalistic (like mathematics, of course) and must rely on its logical linear algebra structure, which is sufficient to describe its approach to stocks and flows (see Bouinot 1972; Kemeny, Schleifer, Snell, and Thompson 1962).

The second part of the book is devoted to the morphogenetic analysis of the performance and the author begins by reminding us that "morphogenesis is the study of the process of artificially creating optimized forms" (p. 151), but again he does not go far enough and does not explain what is an optimal form, and how to generate it.

Although he cites A. M. Turing (1952), he does not go into enough detail to explain the concept of optimal mathematical forms and their application to accounting or—if they exist—the principle of optimal arithmetic. The examples he gives, the Wilson model, the breakeven model, the cost-volume-profit model from Harris, and the coding and data processing, are not used to justify a convincing conclusion: "The morphogenetic analysis of the performance structure using the basic accounting equation leads to develop a generic algorithm the factor values of which are constant in all aesthetic accounting and financial building" (p. 205).

The main objective of the book, which is to show how basic accounting explores the performance in organizations with a normative approach, has not been fully achieved, in my opinion, for two main reasons: first, because it is the nature of basic accounting that has been imperfectly described, and, second, deriving from the first, because this imperfection prevents the author from presenting the very foundations of basic accounting. The interest of this book is in presenting these issues in a new way, and to recall some references to the source of basic knowledge. Everyone can also renew his personal thinking.

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RICHARD M. S. WILSON (editor), *The Routledge Companion to Accounting Education* (Abingdon, Oxon, U.K.: Routledge, 2014, ISBN: 978-0-415-69733-0, pp. xxxviii, 760).

Accounting education is a topic of importance and relevance to all those touched by it, including teachers, researchers, students, and professionals. Thus, *The Routledge Companion to Accounting Education* as a reference that presents an overview of the accounting education subject area makes a valuable contribution. The intent of the volume is to be a reference work that identifies the content area of accounting education, aids accounting education scholarship, and ultimately supports innovation and improvement in accounting education.

The 760-page volume falls squarely within the category of a reference work. Richard Wilson, as editor, with the assistance of Ralph Adler, Sue Ravenscroft, and David Stout as senior associate editors and seven associate editors, have assembled 30 chapters divided into seven sections, with each section introduced by a preface that sets up the collection of chapters. The series of reference books to which this volume belongs is described as presenting a range of international perspectives. The current volume does not disappoint in that regard, as the 67 authors represent 24 countries. Most of the chapters include international or cross-country value, with some specifically focused on providing a global view. Comprehensive coverage in a single anthology of a topic as broad as accounting education is not achievable. But, in combination, the chapters and references cited within these chapters should provide any reader with sufficient resources to embark upon a study of accounting education or accounting education scholarship. The volume ends with three appendices that pay tribute to pillars of accounting education: Will Baxter, Ray Chambers, and Stephen Zeff. The appendices motivate a sense of history and pride regarding the leaders of accounting education that should be of interest to all accounting educators, regardless of whether their research extends to the scholarship of accounting education.

The seven sections effectively organize the chapters into the following topic areas: cornerstone issues, students, curricula, pedagogy, assessment, context issues, and institutions. The approaches of the individual chapters vary widely among facts and analyses, history and projections, research, and professional positions. Despite the differing approaches and content, all of the chapters are well organized and consistently well written. In general, the individual chapters are effective at directing the reader to other chapters within the volume that address related content. As experts, many of the authors rely heavily on their own prior work as they fit their messages within the bigger picture of accounting education developments or literature. Several chapter authors are guilty of over-reliance on personal prior work, but this is not a pervasive problem.

A reader can generally assess interest in a chapter's content based on the section in which the chapter is grouped and the section's preface. While the massive nature of the volume precludes addressing all chapters, what follows are comments on selected chapters. Specifically, comments relate to chapters that may be particularly useful from the viewpoints of research, history, international knowledge, and contemporary topics.

In Chapter 1, "Accounting Education as a Field of Intellectual Inquiry," Timothy J. Fogarty kicks off the volume by describing accounting education and accounting education literature within the realms of economic, psychological, and sociological thought. References to published sources are selective rather than comprehensive, but still give the reader an appreciation for the complementary and competing fields of study on which accounting education is based. Most noteworthy in this chapter is Fogarty's positioning of accounting education research within the hierarchical-status structure of accounting research. His view is dark but realistic in explaining that accounting education research has not achieved high status. His analysis concludes that all accounting educators should share an interest in accounting education and, consequently, that accounting education research should enjoy elevated status. He discusses the impacts of journal space, perceived journal quality, data availability, research methods, and theory building on the status of accounting education research.

In Chapter 6, "Perceptions of Accounting," Ursula Lucas and Rosina Mladenovic provide a comprehensive background to research addressing students' perceptions of the field of accounting, including research originating in several countries. The chapter also actively links accounting education to the scholarship of teaching and learning in higher education. Not surprisingly, the authors report research indicating that many students have a negative or unflattering view of the accounting discipline. They also report studies addressing the impact of students' perceptions on learning, and ways to change students' views.

The strength of this chapter is in providing the reader with an in-depth introduction to a particular area of accounting education research and an entry point into the scholarship of teaching and learning.

Chapter 7 is entitled "The Role of Prior Accounting Education and Work Experience." Like Chapter 6, it makes a significant contribution by providing an in-depth background of a specific area of accounting education research. Marann Byrne and Pauline Willis discuss the streams of research that have explored prior academic and work experiences as influences on students' academic performance. The breadth of studies included in this chapter promises to be very useful to any accounting education researcher who is new to the topic area. Again, the attention paid to including research from various countries enhances the comprehensiveness and value of the chapter.

Chapter 9 is "The Choice of Accounting as a Study Discipline." A concern with this chapter, authored by Fawzi Laswad and Lin Mei Tan, is that its title may deter readers from appreciating the topic it addresses. To clarify, this chapter is a valuable resource for anyone wishing to conduct research that addresses why students choose accounting as an academic major. Another concern is that readers will discover the extremely effective table summarizing research on the topic area from 1982 through 2012 and fail to read the rest of the valuable material included in the chapter. Subsequent to the research summary table, the chapter presents theories, variables, and less well-defined streams of research (for example, personal attributes and job satisfaction). In its concluding section, the chapter presents possibilities for future research, clearly distinguishing questions that have been thoroughly addressed from those that are still open. Material in this chapter has the potential to lighten the workload of any researcher exploring the choice of accounting as an academic major.

Chapter 11 is "The First Course in Accounting." Donald E. Wygal's chapter is grouped within the curriculum section but offers a great contribution in its comprehensive presentation of perceptions and progress of introductory accounting over time, in other words, a historical perspective. The chapter discusses professional and academic reports in the U.K. (Solomons 1974), Australia (Mathews 1990) and the U.S. (The Bedford Committee 1988; *Perspectives on Education* 1989), Accounting Education Change Commission's position and issues statements, and moves on to academic research. Wygal reviews academic research on elementary accounting in New Zealand, Scotland, Australia, Malaya, Hong Kong, the U.K., and the U.S., following the chronological progression of the research from descriptives to interventions focused on course redesign. The chapter concludes with an emphasis on the importance of the first accounting course as a student's gateway to the accounting profession.

Chapter 4 is "The Case for Change in Accounting Education." Although not juxtaposed with Chapter 11, Barbara Flood's chapter includes an extended discussion of the same professional and academic reports addressed by Wygal. She adds the U.S. Pathways Commission Report (Behn et al. 2012) and the Albrecht and Sack (2000) monograph, the Australian Crossroad publication (Evans, Burritt, and Guthrie 2010), and various research and other publications addressing the controversy of the extent to which accounting education succeeds and fails. The value of this chapter is its portrayal of the internal criticism, both constructive and otherwise, of accounting education by accountants in all aspects of the profession. While not a glowing report, it documents the accounting field's historical attempts to critique and improve accounting education. Readers who are unfamiliar with the reports will benefit from the effective summaries.

Chapter 27 is "Fifty Years of Change in Accounting Education: The Influence of Institutions." Gary L. Sundem rounds out the historical view of accounting education, reaching back to early reports on business education by the Ford Foundation and Carnegie Commission published in 1959, and more recent activities by the International Accounting Education Standards Board and International Association for Accounting Education and Research. Sundem's discussion focuses on the impact of institutions. He presents the substance of professional and academic reports, the impact of legislation, educational oversight issues, globalization forces, and even underfunding as influences on accounting education. Sundem's prose, coupled with his reference list, provide a condensed but comprehensive snapshot of 50 years of external influences on accounting education.

Chapter 15 is "Emerging Areas within the Accounting Curriculum." This chapter is unique to the volume in its data-gathering approach. Alan Sangster collects and presents the comments of 21 accounting educators from different locations around the world. Comments originate from, and are grouped into, the following regions: The Middle East, Africa, The Far East, Australasia, Latin America, North America, and Europe. The commenters generally focus on the state and methods of accounting and accounting education, accounting and

auditing standards, economic influences, and other external events important to accounting education in their respective countries. Even though the information reported in this chapter will eventually become dated as circumstances in the represented countries change, readers are given the opportunity to assess whether their understandings and expectations of accounting in various locations around the world are correct and current. Researchers intending to incorporate multi-country considerations in their designs can get at least a glimpse of factors they might need to understand as they develop background knowledge and identify variables. Beyond the research benefit, the information is interesting and broadening for those who have a desire to understand accounting and accounting education in parts of the world other than their own.

In Chapter 26, "Comparative Accounting Education," Charles H. Calhoun and Gert H. Karreman augment the contribution of Chapter 15 in that they fill in international information and the facts about various countries in terms of accounting education journals, standards, standard setters, legal structures, professional accounting organizations, and licensure. Although the content is not as intriguing as that of Chapter 15, when the two chapters are read together they provide significant background for any reader interested in accounting education and professional preparation in countries around the world.

Chapter 21 is "Outcomes Assessment in Accounting." Even academics well versed in assessment will likely gain knowledge from the discussion of the historical context of higher education assessment provided by Linda A. Kidwell and Suzanne Lowensohn. For any who are not yet steeped in assessment process and requirements, the chapter provides a context for why and how the assessment of learning has evolved, as well as explanations and examples of basic components of assessment such as learning objectives and measurement techniques. As noted by the authors, the chapter is somewhat U.S.-centric, a characteristic they attribute to the abundance of information readily available about assessment in the U.S. Ph.D. students and new accounting faculty entrants who have not yet been involved in assessment reporting and accreditation processes may benefit the most from this chapter. Since one of the volume's goals is to identify the content area of accounting education, this chapter makes a vital contribution.

Chapter 24, "Ethics and Accounting Education," is Gordon Boyce's chapter on the role of ethics in accounting education. It is more normative than the other chapters discussed in this review in that it presents a case for the need to incorporate ethics into accounting education. Like Chapter 21, it is an important component of a volume intended to identify the content area of accounting education. Strengths of the chapter include its historical and contemporary viewpoints, along with its broad background presentation that culminates in specific details related to ethics in accounting education. Boyce includes significant discussion on teaching tools making the information content applicable to teaching activities.

Chapter 5 is "An Agenda for Improving Accounting Education." E. Kent St. Pierre and James E. Rebele author a chapter frequently referenced by other contributors with a phrase similar to "For a contrary view, see Chapter 5." Parallel to Fogarty's rather pessimistic view of the status of accounting education research in Chapter 1, St. Pierre and Rebele describe a pessimistic view of the value of accounting curricula that elevate soft skills in an environment in which students' educational exposure is resource constrained. They believe the limited time that students have to learn the vast subject of accounting is best spent developing technical accounting knowledge and expertise. Lack of evidence that, for example, writing and ethics can be effectively taught to college juniors and seniors by accounting academics who are not trained in how to teach those disciplines underpins the argument that the time available in accounting courses should be devoted to the mastery of accounting skills. This chapter is refreshing in its underlying message urging accounting academe to think critically about how to best educate accounting students.

Multiple audiences may derive value from this collection of chapters on accounting education. Ph.D. students and accounting education researchers will likely benefit the most. Researchers, particularly those new to either accounting education research generally or to a specific topic, can use these chapters and their citations to gain access to the accounting education research literature. Ph.D. students can use the volume to gain a fairly comprehensive general knowledge of historical and contemporary accounting education topics, including research published to date. Improvement in accounting education, a goal of the volume, may be an ultimate result, though an indirect one, as the collection is not particularly focused on helping teachers teach. Rather, its primary contributions are enhancing understanding of accounting education and supporting research. The editor has assembled chapters from an extremely knowledgeable set of authors, producing a very useful information source on a wide array of accounting education topics.

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GUOCHANG ZHANG, *Accounting Information and Equity Valuation: Theory, Evidence, and Applications* (New York, NY: Springer, 2014, ISBN 978-1-4614-8159-1, pp. xxiv, 233).

This book is largely drawn from the author's previously published papers on the real option-based valuation model (ROM), which the author uses as a unified theory to integrate and explain a variety of empirical findings in the value-relevance literature. The book starts with the residual income model. Revived by Ohlson (1995) and Feltham and Ohlson (1995), the residual income valuation method (RIM) has gained prominence in academic accounting over the past two decades. The attraction of RIM to academics arises for both theoretical and empirical reasons. On the theory side, RIM is algebraically isomorphic to the discounted dividend model. It exhibits the dividend displacement property of Modigliani and Miller (1958, 1961). It focuses on value creation rather than on value distribution. By moving away from pure cash accounting, it nests the discounted cash flow model within it as a special case. In terms of empirics, among other benefits RIM offers a way to legitimize the use of cross-sectional "price levels" regressions.

Following the linear information dynamic approach, Ohlson (1995) shows that equity value is a linear function of the book value of equity and earnings. As book value and earnings are two summary measures applicable to common shareholders from the balance sheet and the income statement, respectively, the Ohlson model links equity value to accounting numbers in the financial statements, an approach intuitively appealing to accounting academics and practitioners. In addition, the Ohlson model is attractive because it integrates two heuristic, yet popular, valuation approaches: the price-book and price-earnings regressions in academics and the price-to-book and price-to-earnings multiples in practice.

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In Chapter 3, the author provides a model illustrating that capital follows profitability, indicating that the residual income information dynamic is nonlinear. This nonlinear information dynamic opens the door to the real options-based valuation model in Chapter 4. Intuitively, firms adjust their capital investment on the basis of profitability. When profitability is very low, firms have the option to shut down their operations, called the put option in the book, and return existing capital to investors. In this case, the book value of equity proxies for existing capital and thus determines the value of the put option. When profitability is very high, firms have the option to expand their operations, termed the call option in the book, and generate future earnings that are much higher than current earnings. Profitability drives capital investment and thus determines the value of this call option.

The put and call options introduce nonlinear relationships between equity value and accounting variables (book value and profitability). Equity value is determined by book value when profitability is very low and is increasingly determined by profitability when profitability increases. In this framework, the linear information dynamic becomes a special case of ROM when a firm is in a steady state—that is, when both put and call options have no value. In Chapters 6 and 10, the author shows that many empirical findings in the value-relevance and return-earnings literatures can be reconciled with ROM. Finally, the author applies the real option-based approach to segment reporting in Chapters 7 and 8 and to industry-level competition in Chapter 12.

The book has a number of excellent features. First, I like the fact that the author first offers a theory and then empirically tests its implications. In the accounting field, theoretical and empirical research have become two separate lines of investigation with little overlap. When I was a Ph.D. student, a professor once told me that he had submitted a paper with both a theoretical model and empirical tests. One referee told him to focus on the theoretical part and to remove the empirical tests, while the other referee suggested removing the theoretical discussion while expanding the empirical part. Although this example might be somewhat extreme, I do believe that theoretical and empirical accounting research have become progressively disconnected, and this book offers a direction for a return to a merging of theoretical and empirical research.

A second appealing feature is that the theoretical model not only explains existing empirical findings in the literature, but also generates additional empirical predictions. For each theoretical model, the author offers empirical tests of the model's predictions. Empirical evidence consistent with the predictions validates the theoretical model. In addition, the author shows that ROM could be a unified theory to explain a variety of empirical findings documented in the literature.

Finally, I believe that both call and put real options are relevant for firm valuation. In practice, the existence of the put option is evident for loss firms. When earnings and cash flows are negative, the firm's value is largely determined by its financial condition (balance sheet variables in general and the book value of equity in particular). In contrast, the call option is the main driver of valuation for high-growth firms, whereas both book value and current earnings play only a limited role. Given the variations in profitability and growth potential across firms, ROM offers a better description of the cross-sectional variation in equity value than does the linear information dynamic in the Ohlson model.

I would like to see more coverage or a more balanced discussion of several issues, which in my view could improve the usefulness of the book to both practitioners and academics. These issues indicate the limitations of the book but at the same time represent fruitful avenues for future research. Three subjects in particular deserve a more extended treatment: the usefulness of the book for practitioners, the implications of ROM for investor behavior and market efficiency, and the implications of ROM for standard setters.

The first issue relates to how this book can improve practice in the real world. The main capital-market users of valuation models are analysts issuing stock recommendations and investment banks supplying fairness opinions to target shareholders in corporate acquisitions and change-of-control transactions, such as management buyouts. Unlike academics, analysts and investment bankers do not run cross-sectional regressions of equity value on book value and earnings. Instead, they typically construct pro forma financial statements and forecast future earnings and cash flows for many years, following the approach taught in M.B.A. valuation courses (Lundholm and Sloan 2013; Penman 2012; Easton, McAnally, Sommers, and Zhang 2014). How ROM can improve the valuation exercise is unclear, especially above and beyond the traditional residual income model. Hand, Coyne, Green, and Zhang (2014) show that the median length of the explicit forecast horizon for ROE-based residual income valuations is 19 years (ten years for discounted cash flow models). As analysts forecast investment and growth in detail each year during the forecast horizon, the option value embedded in ROM is implicitly captured in pro forma financial statements during the forecast horizon.

Both Ohlson's linear information dynamic and this book's ROM are simplified ways of using current financial numbers to project future financial numbers. Once detailed pro forma financial statements are available, neither the linear information dynamic nor ROM is of much value in projecting future financial numbers, except that the linear information dynamic applies in the calculation of the terminal value.

The residual income model has a number of advantages relative to discounted cash flow models: (1) book value as a large chunk of the value estimate is observable and thus free of estimation error; (2) only the cost of equity is needed to calculate the discount rate, a feature that is especially appealing for firms with a changing and/or complicated capital structure; and (3) the time-series of abnormal earnings and return on equity offer a reality check since return on equity should converge toward the cost of capital in equilibrium.[1] However, residual income models account for only about 5 percent of valuation models used by sell-side analysts, whereas discounted cash flow models are used by 95 percent of analysts (Hand et al. 2014).[2] In addition, the 5 percent use is largely attributable to a single brokerage firm, Morgan Stanley, and arose after it hired accounting academics such as Trevor Harris and Peter Joos to be in charge of sell-side analysts. Given the superiority of residual income valuations in many settings, accounting academics find the failure of practice to widely adopt this technique both puzzling and frustrating. This matter deserves more thought from readers of this book, and one hopes that future research will facilitate the adoption of residual income valuation in practice.

The second issue relates to the implications of ROM for academic research, especially in the context of investor behavior and market efficiency. Both the Ohlson model and ROM express equity value as a function of accounting variables. Relative to the Ohlson model, ROM has more flexibility in accommodating distressed and high-growth firms. These models help explain how investors price fundamental information as reflected in accounting variables. However, like the Ohlson model, ROM does not reveal whether investors price such information efficiently. The main objective of the capital markets is to allocate scarce capital resources to their most productive use. If ROM helps to identify any price inefficiency, then it could play an active role in the capital resource allocation efficiency. The nature of this active role remains elusive, as the incremental information identified in ROM does not seem to support this active role. For example, a key contribution of ROM beyond the linear information dynamic in the Ohlson model is to introduce a call option for high-growth firms. If investors under-react to such a call option prior to the publication of ROM, then high-growth firms should have higher subsequent stock returns when the call option value is reflected in earnings and book value. However, prior studies find that, if anything, high-growth firms tend to have lower future stock returns (Lakonishok, Shleifer, and Vishny 1994; Zhang 2007). How to use ROM to identify price inefficiency seems to be a fruitful avenue for future research.

The last issue relates to the implications of ROM for standard setters, which the author discusses extensively. Although the author emphasizes that such implications are only from the valuation perspective, I think the drawing of implications of ROM for standard setters needs to be done with care. As Holthausen and Watts (2001) discuss, documented associations between accounting variables and equity value have limited implications or inferences for standard setting because they are merely associations. Other than equity valuation, accounting standards need to accommodate a variety of demands, such as executive compensation and debt contracting. Inferences are difficult to make from the equity valuation perspective alone. More importantly, ROM and many other models implicitly assume that the capital markets are efficient. If the stock price is right, then why bother to incorporate value-relevant information in the accounting system from the valuation perspective, since this information is already reflected in the stock price? An extreme case of fair value accounting illustrates this point: if the main objective of standard setters is to increase the value relevance of accounting numbers as reflected in the R^2 in the price or return regressions, then we can simply take the market value of equity at the end of the fiscal period as the book value and take changes in market value during

[1] Hand et al. (2014) show that residual income valuations that are based on net income are reliably smaller than their discounted cash flow counterparts, are unbiased relative to future prices, and contain forecasted future returns on equity that decline toward a terminal year median of 17 percent. In contrast, DCF valuations and residual income models derived from DCF have embedded forecasted returns on net operating assets that increase toward a terminal year median of 28 percent, which seems unrealistic.

[2] How widespread the use of residual income models is among other capital-market users, such as investment bankers, is unknown because such information is not publicly available.

the fiscal period as net income. This is an accounting system with 100 percent fair value accounting. In this scenario, we can get an R^2 of 1 in both price and return regressions. It immediately becomes clear that such an accounting system is undesirable and provides no incremental information beyond the observable stock price. Echoing the point that I made in the last paragraph, I think that ROM can make more important contributions if it goes beyond market efficiency and helps to identify whether investors price accounting variables efficiently.

Overall, this book should be very useful for academics who are interested in valuation studies. It provides an excellent framework for the relationship of accounting variables to equity value. It also contains a lot of food for thought and potential topics for future research.

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Yale University

Closing Note

Books are an indispensable part of the accounting research literature, and each year many new and interesting books appear on the scene, a considerable number of which are published in the U.K. A book review section has appeared in *The Accounting Review* ever since its first issue in 1926—apart from a brief hiatus between 2003 and 2006. Since the resumption of the section in 2007, when I began as its editor, *The Accounting Review* has published the reviews of 129 books. This number excludes educational textbooks, which are reviewed in *Issues in Accounting Education.*

As the American Accounting Association is a truly international organization, I have deliberately assigned just over half of the books to reviewers based outside the United States, drawn from a total of 17 countries. The range of coverage has been wide: empirical and analytical research in financial and management accounting, historical research, and research on auditing, taxation, standard setting, corporate governance, sustainable reporting, and corporate social reporting. Several of the books were not in English, so that we might become apprised of work done in other languages. In order to widen the net of candidate reviewers, I have challenged myself to use a particular reviewer no more than once: no one has reviewed more than a single book. For a number of books that did not justify a full review but otherwise deserved to be brought to the attention of readers, I have written a number of "capsule commentaries," as I did back in 1962–1966, when I served previously as *The Accounting Review*'s book review editor.

Not all of the reviewers have been accounting academics. Two have come from sociology and law, one was a leading financial journalist, and another was a senior staff member of the IASB.

I wish to thank the succession of my Senior Editors—Dan S. Dhaliwal, Steven J. Kachelmeier, John Harry Evans III, and Mark L. DeFond—for their support of the book review section.

This will be my final issue as book review editor. I am very pleased to pass the baton to my successor, Gary C. Biddle.

—Stephen A. Zeff
Rice University

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

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The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

Format

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3. Margins should be at least one inch from top, bottom, and sides.
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5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.
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Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: three days; 3 kilometers; 30 years. All other numbers are expressed numerically.

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An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. The Keywords statement should appear immediately below the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

WESTERN WASHINGTON UNIVERSITY invites applications for tenure-track Assistant or Associate Professor position in Accounting. Flexible appointment date: Fall 2015 or later. Auditing, financial, or accounting systems preferred; other specialization areas may be considered. Doctoral degree in accounting or equivalent, completed or nearing completion. Candidates appointed ABD must complete degree requirements by June 15 of the first year. Must qualify as AACSB Scholarly Academic (or will qualify upon completion of doctorate). Strong teaching/research record or clearly demonstrated potential required. Duties include teaching undergraduate/graduate courses, university service, and scholarly publication. Western is on the coast between Seattle and Vancouver, BC; outstanding recreational areas nearby. WWU has 14,900 students; the college is AACSB-accredited. Applications must include application letter (include how qualifications are met), *curriculum vitae,* evidence of teaching effectiveness, transcripts, and names of three references. All application materials must be uploaded through the Electronic Application System for Employment (EASE) at: http://www.wwu.edu/jobs. Application review begins immediately; continues until position is filled. AA/EO.

GRAND VALLEY STATE UNIVERSITY, Seidman College of Business, School of Accounting invites applications for a new position beginning in Fall 2016. The open position is for an Assistant or Associate Professor. Specialization in Auditing, Financial, or Taxation is desirable. Applicants should have a doctoral degree in accounting or other equivalent terminal degree appropriate to teach taxation (such as a J.D.) plus either Master's in Taxation (M.S.T.) or L.L.M. (tax) or M.S.A., and a strong commitment to teaching excellence and scholarship research. Salary is competitive. Visit our website at: http://www.gvsujobs.org/ for more information, additional requirements, a full description of the position, and details on how to apply. The Seidman College of Business and the School of Accounting are both fully accredited by AACSB International, and value and support a blend of excellent teaching and scholarly productivity. The atmosphere at the Seidman College of Business is highly collegial, with opportunities for outstanding professional growth. The Grand Rapids community is a vibrant, exciting area, with significant leisure time activities available. The University and Seidman offer an excellent benefits package. Review of applicants will begin immediately and will continue until the position is filled. Grand Valley State University is an Equal Opportunity/Affirmative Action Institution.

EDINBORO UNIVERSITY OF PENNSYLVANIA is seeking candidates for a full-time tenure-track, Accounting, Open Rank position beginning August 2015 to teach principles of accounting and upper-level courses in accounting based on the needs of the department and the qualifications of the candidate. Preference will be given to applicants who apply by February 23, 2015; however, the position will remain open until filled. For more information or to submit an application, please visit Edinboro University's Human Resource and Faculty Relations office at: http://jobs.edinboro.edu/postings/2439.

MOLLOY COLLEGE, a private college in the Catholic and Dominican tradition, is accepting applications for the full-time faculty tenure-track position of Assistant Professor of Accounting. Duties and responsibilities: Accounting courses to be taught include Principles and Advanced Accounting classes at both the undergraduate and graduate levels. Qualifications: A Ph.D., D.B.A. or D.P.S. is preferred; however, candidates who are ABD or are near completion of their doctoral degree may be considered. Candidates with business expertise and experience in the field of Accounting will be given preference. Evidence of outstanding teaching ability must be demonstrated. To apply, please send a letter of application, *curriculum vitae*, and three letters of recommendation to Email: humanresources@molloy.edu, or mail to: Human Resources, Molloy College, PO Box 5002, Rockville Centre, NY 11571. An Equal Opportunity Employer. Women, minorities, people with disabilities, and veterans are encouraged to apply.

FAIRLEIGH DICKINSON UNIVERSITY, Silberman College of Business, Department of Accounting, Taxation and Law is accepting applications for a tenure-track position in Accounting at the Assistant/ Associate Professor rank for Fall 2015. Candidates should have a Ph.D. in an Accounting from an AACSB-accredited institution with evidence of or potential for high-quality teaching and scholarship. The candidate will be expected to teach undergraduate- and graduate-level accounting courses. The normal teaching load is 9 credit hours per semester if engaged in research. Salary and benefits are competitive with AACSB-accredited institutions. Professional certification (CPA or similar) and relevant professional work experience are desirable. The College offers undergraduate and graduate programs that have earned AACSB-accreditation. The College has been consistently included by the *Princeton Review* in its listing of Best Business Schools in the country. The department offers a combined B.S./M.S. in accounting program (winner of the AAA's Innovation in Accounting Education Award), as well as an M.S. in accounting program for those without an undergraduate accounting background. In recent years, some of the highest scores on the NJ CPA licensing exam have been posted by our graduates. The department's M.S. in Taxation program is one of the oldest and largest programs in the state. The department has more than 450 students enrolled in its various programs. To apply, Email: Dept. Chair west@fdu.edu

CLAYTON STATE UNIVERSITY, College of Business (AACSB-accredited) has a job opening for a tenure-track Assistant Professor of Accounting, beginning August 2015. The ideal candidate will have an earned Ph.D. in Accounting (CPA desirable; ABDs with definite defense date will be considered). The successful candidate will teach individual and corporate income tax, and intermediate accounting, in addition to accounting principles courses. Opportunities exist for graduate teaching. Research and service requirements are competitive with other AACSB-accredited institutions. Clayton State University, a campus of the University System of Georgia, is located in a park-like setting surrounded by lakes in south suburban Atlanta. The College of Business currently offers an undergraduate B.B.A. major in Accounting, and an M.B.A. degree in Accounting. Qualified candidates should visit: https://clayton.peopleadmin.com/postings/4191 to apply online. For more information on the College of Business, please visit: http://www.clayton.edu/business.

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